Ruifang Ma
Université Paris-Saclay, Laboratoire GEOPS, UMR 8148
Campus scientifique d'Orsay, Bâtiment 504
91405 Orsay, France
maruifang89@hotmail.com

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Dear editor,

We have submitted a new version of manuscript online, taking into account most of the comments made during the review process. First, we would like to thank you for your help in each step of the submission and review processes, as well as the constructive comments provided by you and the two reviewers. The main points relied on i) the interpretation of the geochemical proxies, related to changes in the circulation and/or productivity, that should be more critical and ii) a better assessment of the water masses at studied sites (and, especially, the occurrence of North Atlantic Deep Water NADW). We reinforced these two points in the new version of the manuscript, following the strategy suggested by both reviewers.

In details, we answered to the main comments about the uncertainties on the interpretation of the Cd_w values, especially during the last deglaciation. We improved this point by developing the discussion about the Cd_w as a proxy for the productivity changes and changes in the circulation (see details in the following answer to the reviewer as well as the new paragraph in the revised manuscript in section 5.4, lines 500-517). To better assess the contribution of mixed various end members for the deep water in Indian Ocean, we developed the discussion about deep water masses in modern period (section 2, lines 123-125) and also the contribution of NADW at the studied sites during the Holocene in the revised manuscript (section 5.2, lines 359-365 and 405-409). As previous studies based on multiple geochemical proxies (benthic $\delta^{13}C$ values, B-P age offsets, ε_{Nd}, $\delta^{18}O_{ivc}$ and Mg/Li) obtained from same cores and/or near core sites suggest an increased influence of NADW during the Holocene (Ahmad et al., 2008; Raza et al., 2014; Yu et al., 2018; Naik et al., 2019; Ma et al., 2019;

2020), we still believe in our original interpretation, but we reinforced the discussion, as developed in the following detailed answers.

The second reviewer, Dr. André Bahr, also underlined some confusion about the way to interpret elemental ratios during the last deglaciation. To correct that, we developed the interpretation of the comparison between Cd_w and *G. bulloides* abundances (section 5.4, lines 500-517), to reinforce the discussion about the influence of changes in water masses and/or ventilation at the studied site during the last deglaciation. Moreover, most of the other comments suggested by Dr. André Bahr were taken into account into the final version of the manuscript, as detailed below.

To finish, we decided to add the anonymous reviewer#1 as well as André Bahr in the 'Acknowledgements' part for their help to improve the quality of the manuscript.

We hope that the new version of the manuscript meets the high-quality standards set for *Climate of the Past* publications.

In the following, we provide detailed answers for the specific reviewer comments.

Reply to reviewer #1 comments:

We thank Reviewer 1 for providing helpful comments on our manuscript, they have been carefully considered. Please find below our answers to these comments.

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 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 Ka BP) 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 are 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 Ka BP? 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 Ka BP 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.

Answer: The two main comments made by the first reviewer concerns i) the influence of surface productivity changes on the intermediate Cd_w records at millennial time scale during the last deglaciation (16-16.5 cal kyr BP) and the Holocene (2-1.5 cal kyr BP) and ii) the contribution of intermediate water circulation variations, especially during the Holocene. We fully agree with the reviewer that there may be some questions interpreting the Cd_w records, especially at the millennial scale. Changes in intermediate Cd_w values of benthic foraminifera can be influenced by different processes such as surface productivity, changes of the water mass sources and/or ventilation (e.g., Came et al., 2008; Bostock et al., 2010; Olsen et al., 2016; Poggemann et al., 2017; Yu et al., 2019). First, at the Arabian Sea site, we suggest that the observed significant increase of intermediate Cd_w values from the last deglaciation (~0.7 nmol/kg) to the late Holocene (~1.59 nmol/kg) could be associated with the surface productivity; indeed, this is supported by the *G. bulloides* record from the same core and another one close to the studied site (Bassinot et al., 2011; Naik et al., 2017), as well as by previous studies suggesting that increased Cd_w values (>1 nmol/kg) could correspond to elevated surface productivity (Bostock et al., 2010; Olsen et al., 2016).

However, we also agree with the reviewer that there is some mismatch between the increased *G. bulloides* abundances and the decreased Cd_w values obtained from the same core MD77-191 during the last deglaciation, especially at around 16-16.5 cal kyr BP. As the resolution of both records from core MD77-191 is relatively low during YD and HS1, it seems more reasonable to use the high-resolution *G. bulloides* abundances records from the near core site SK237 GC04 (1245m, southeastern Arabian Sea, Naik et al., 2017), reflecting the surface productivity changes in this area. Therefore, we observe that although the *G. bulloides* abundances from the Arabian Sea display an increasing trend during HS1 and YD events, these modest increases in surface productivity are synchronous with low Cd_w values (~0.6 nmol/kg) during HS1 and YD, much lower compared to the Cd_w results during the late Holocene (~1.59 nmol/kg). Thus, we suggest that the variations of surface productivity could not be the main control at this studied site during the last deglaciation, and thus we proposed that changes in the circulation can explain the observed results. Indeed, the influence of intermediate water masses variations has also been demonstrated by many proxies (ε_{Nd}, benthic $\delta^{13}C$, B-P age offsets and CO_3^{2-}) in the northern Indian Ocean (Bryan et al., 2010; Yu et al., 2018; Ma et al., 2019; 2020). These previous studies have been summarized and detailed discussed in the manuscript (lines 465-507). Consequently, the conclusion that the influence of changes in water masses and/or ventilation on Cd_w records from the northern Indian Ocean during the HS1 and YD may be still reasonable. In order to take this comment into account, we modified the discussion in section 5.4 (lines 500-517) as below:

Significant decreases in *G. bulloides* relative abundance of cores SK237 GC04 (Naik et al., 2017) and MD77-191 records were observed from the HS1 to B-A (Bassinot et al., 2011), and thereafter slight increases occurred in the YD (Fig. 5). These high values at both core sites during the HS1 and YD may indicate an enhanced surface productivity during these intervals (Fig. 5). This should have led to increased intermediate Cd_w and organic matter preservation under low oxygen concentration conditions during the HS1 and YD. However, despite a low resolution for the MD77-191 Cd_w record during the last deglaciation, we do not observe high values of intermediate Cd_w during the HS1 and YD (~0.6 nmol/kg) compared with the late Holocene (~1.59 nmol/kg), especially at 16.5-16 cal kyr BP. Although we cannot fully discard the influence of surface productivity on the intermediate Cd_w in these time intervals, this apparent discrepancy seems to provide another evidence for the influence of changes in water masses and/or ventilation during the HS1 and YD, as already demonstrated by previous studies and proxies in the northern Indian Ocean (Bryan et al., 2010; Yu et al., 2018; Ma et al., 2019; 2020).

We also would like to thank the reviewer to point out that the influence of water mass ventilation on the intermediate Cd_w during the Holocene should be better discussed. In order to clarify this point, we added some new parts in section 5.2 (lines 359-365, 405-409) about the possible contribution of past water masses changes to the Cd_w records obtained from northern Indian Ocean during the Holocene. Briefly, increased benthic $\delta^{13}C$ values and B-P age offsets, as well as depleted ε_{Nd}, $\delta^{18}O_{ivc}$ and Mg/Li results obtained from MD77-176 and MD77-191, suggest the increased influence of NADW (Yu et al., 2018; Ma et al., 2019; 2020); indeed, deep-water masses can contribute to intermediate water masses in the Northern Indian Ocean by upwelling when flowing northward (Talley et al., 2011; Naqvi et al., 1994). NADW is characterized by the fresh, well-ventilated and depleted nutrient (modern Cd_w, ~0.2 nmol/kg; Poggemann et al., 2017), which is also in good agreement with the benthic assemblage analyses from the same cores. Therefore, although it may be difficult to exclude the influence of NADW during the Holocene, the significant high intermediate Cd_w during the late Holocene does not correspond to the increased contribution of NADW, suggesting that our initial interpretation could be also maintained during the Holocene.

b) Also, in line 329 (and thereafter), the authors, for the first time, mention NADW, claiming that this water mass would dominate during the early Holocene at site MD77-191. How does this claim compare to the modern water mass distribution in the area? Is it not true that most of the deep waters in the Indian Ocean are mixes of various end members, 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 deep water 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.
Answer: We fully agree with the reviewer that the discussion about the NADW has to be improved. In the modern northern Indian Ocean, the Indian Deep Water (IDW) lies between 1500 and 3800 m. The IDW forms from the mixing between NADW and Circumpolar Deep Water (Talley et al., 2011). As already detailed in the answer to the upper comment, multiple geochemical proxies obtained from core MD77-191 (Arabian Sea) and MD77-176 (northern BoB) as well as previous studies (Yu et al., 2018; Ma et al., 2019 and 2020) have provided strong evidence for the increased contribution of well-ventilated NADW during the Holocene. Thus, although the influence of NADW on the intermediate Cd_w cannot be fully discarded during the Holocene, our records suggest the dominated role of surface productivity in controlling Cd_w records during the Holocene, and thus, our conclusion will not change but will be better discussed (the modifications of NADW discussion are explained in Reply a)); indeed, in order to take into account these comments, we developed the discussion about NADW during the modern and Holocene periods (section 2, lines 123-125).

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.
Answer: We have corrected this mistake in the revised manuscript (line 152) to be:

C. pachyderma is a shallow infaunal species.

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.
Answer: We agree that these sentences are not well-written because we only focused on the long-time scale variations, so we used this comment to improve the following discussion of the comparison. Please see lines 397-403.

Despite a lower resolution for MD77-191 *H. elegans* Cd_w records, when compared to the C_{org} and the *G. bulloides* percentage from core SK237 GC04, all of them seem to exhibit similar trends at the long-time scale from the last deglaciation to Holocene; however, some little discrepancies can be observed at millennial time scales, especially during the late Holocene (Fig. 5).

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.
Answer: Thanks for this reminding. We have detailed these records explanation in the revised version (lines 481-486) as the following paragraph, especially for figures 6 d-f records.

These changes are consistent with the weakened the summer monsoon intensity, with less rainfall during the late Holocene, as observed in the BoB using core MD77-176 seawater $\delta^{18}O$ and core SO188-342KL δD_{Alk-ic} records (Marzin et al., 2013; Contreras-Rosales et al., 2014; Figs. 6 e-f). In addition, this is also strongly supported by the $\delta^{13}C_{wax}$ records from the Lonar Lake over the Indian continent (Sarkar et al., 2015; Fig. 6d) and a progressive increase in monsoon summer winds to the South of India (Bassinot et al., 2011).

Besides, in the figure and caption, we also made the brief description.

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.

<u>Reply to reviewer #2 comments:</u>

We thank Reviewer Dr. André Bahr for providing helpful comments on our manuscript, they have been carefully considered. Please find below our answers to these comments.

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).

Answer: The reviewer suggests that the arguments for the monsoon intensity influence on the PP in the southeastern Arabian Sea and northeastern BoB during the Holocene should be stronger. In order to reinforce the discussion, we developed this part in section 5.3 (lines 447-451), improving the quality of the discussion about the contribution of nutrient inputs from rivers in these areas, even if this process seems not to be significant. We are now providing the following explanation in the manuscript:

However, the distribution of chlorophyll in surface water of the western BoB suggests a low annual productivity, indicating that the BoB is not significantly influenced by the riverine nutrient input (Zhou et al., 2020). Thus, it is likely that this increase in fresh water drove pronounced ocean stratification in the northeast BoB, which could impede the nutrient transfer from deep layer to the euphotic upper seawater column, and then inducing low productivity.

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 $\delta^{13}C$ 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.

Answer: We fully agree with the reviewer that the conclusion - *H. elegans* Cd_w displays an anti-correlated compared with the *G. bulloides* abundances during YD and HS1 - seems to be overstated because of the relatively low resolution during the last deglaciation. In order to take this comment into account, we slightly modified the discussion by separating the interpretation of the comparison between Cd_w and *G. bulloides* abundances (revised section 5.4, lines 500-517). We are now providing the following corrections in the manuscript:

Significant decreases in *G. bulloides* relative abundance of cores SK237 GC04 (Naik et al., 2017) and MD77-191 records were observed from the HS1 to B-A (Bassinot et al., 2011), and thereafter slight increases occurred in the YD (Fig. 5). These high values at both core sites during the HS1 and YD may indicate an enhanced surface productivity during these intervals (Fig. 5). This should have led to increased intermediate Cd_w and organic matter preservation under low oxygen concentration conditions during the HS1 and YD. However, despite a low resolution for the MD77-191 Cd_w record during the last deglaciation, we do not observe high values of intermediate Cd_w during the HS1 and YD (~0.6 nmol/kg) compared with the late Holocene (~1.59 nmol/kg), especially at 16.5-16 cal kyr BP. Although we cannot fully discard the influence of surface productivity on the intermediate Cd_w in these time intervals, this apparent discrepancy seems to provide another evidence for the influence of changes in water masses and/or ventilation during the HS1 and YD, as already demonstrated by previous studies and proxies in the northern Indian Ocean (Bryan et al., 2010; Yu et al., 2018; Ma et al., 2019; 2020).

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.

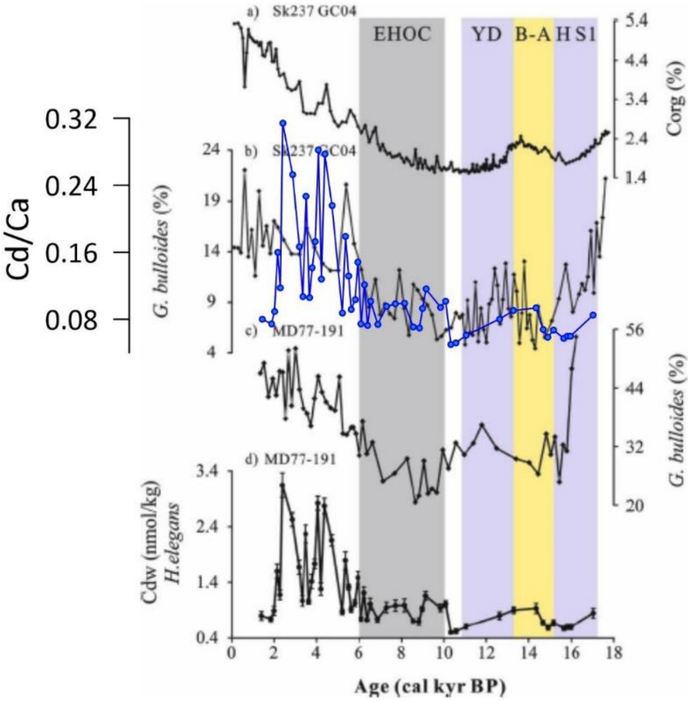

Figure 5 with Cd/Ca from MD77-191 in blue, illustrating that Cd/Ca follows *G. bulloides*-abundances (even more in the more high-resolution data of adjacent core GC04).

Answer: Although the new figure suggested by the reviewer allows directly comparing the Cd/Ca with the % *G. bulloides* record, it may be difficult to decipher both records. Moreover, the reviewer superimposed records from two different cores, even if the global trends are very close to each other. Thus, we prefer to keep the Fig. 5 in its original version.

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.

Answer: We agree with the reviewer and add some sentences (lines 78-87) to clarify the motivation as is shown in the following paragraph.

By contrast, little is known about the paleoproductivity of the BoB, especially its links to changes in monsoon precipitation (Phillips et al., 2014; Zhou et al., 2020). Consequently, studying paleoproductivity and past nutrient concentration of intermediate water masses in the northeastern Indian Ocean will also allow us to completely understand the influence of monsoon climate changes in tropical ocean ecology at different timescales. Besides, as the benthic foraminiferal Cd/Ca is a promising proxy to reconstruct the intermediate-deep water nutrient content (e.g., Boyle and Keigwin, 1982; Tachikawa and Elderfield, 2002; Came et al., 2008; Poggemann et al., 2017; Valley et al., 2017), most of the studies referred to above have reconstructed deep-intermediate water masses in the past (e.g., Came et al., 2008;

Bryan and Marchitto, 2010; Poggemann et al., 2017; Valley et al., 2017), and only few works indicate the relationship between the intermediate water masses nutrient and surface productivity (Bostock et al., 2010; Olsen et al., 2016).

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

Answer: We have corrected this sentence in the revised manuscript (line 93) to be:

"estimate past changes in the nutrient content since the last deglaciation."

l. 100: "of the planktonic…"

Answer: It has been done. Please see line 108.

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

Answer: It has been done. Please see lines 109-110.

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

Answer: Corrected. Please see line 117.

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

Answer: We fully agree this comment and removed it in the revised manuscript. Please see line 127.

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

Answer: Corrected. Please see line 140.

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?

Answer: We have corrected this sentence in the revised manuscript (lines 178-179) to be:

In order to describe major faunal variations, we performed principal component analysis (PCA) on the variance-covariance matrix using the PAST software (Version 3.0, Hammer et al., 2001).

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?

Answer: We fully agree with the reviewer that *H. elegans* is a well-documented and faithful recorder of bottom water Cd/Ca. Indeed, Tachikawa and Elderfield (2002) indicated that due to the lower partition coefficients, the infaunal benthic foraminifera could record Cd/Ca values similar to *Cibicidoides*, despite elevated pore water Cd. Thus, many previous studies used both *Cibicidoides* and *Uvigerina* in paleoceanographic reconstructions (e.g., Marchitto and Broecker, 2006; Makou et al., 2010; Umling et al., 2018; 2019). At core MD77-191 site, we could provide continually calcite benthic species samples with different microhabitat (*Cibicidoides pachyderma*, *Uvigerina peregrina*, and *Globobulimina* spp.). Therefore, we performed Cd/Ca analyses on these benthic species to improve understanding of possible species level differences and microhabitat effects on the benthic Cd/Ca records. We clarified this point in the revised manuscript (lines 149-152) as:

In order to improve understanding of possible species level differences and microhabitat effects on the benthic Cd/Ca records, we analyzed Cd/Ca in three calcite (*Cibicidoides pachyderma*, *Uvigerina peregrina*, and *Globobulimina* spp.) and one aragonite (*Hoeglundina elegans*) benthic foraminiferal species from core MD77-191.

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

Answer: We have corrected this sentence in the revised manuscript. Please see lines 214-215.

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?

Answer: The total variance of other PCs is 49% which is shown in the following figure. Compared with the total variance of PC1 (42%) and PC2 (19%), PC3 is the largest one and only explains 8% of the total variance for the rest PCs. The species composition consists of *Hoeglundina elegans* (0.66), *Globobulimina* spp. (0.22) (Positive loadings), *Uvigerina peregrina* (-0.59), *Cibicidoides pachyderma* (-0.21) (Negative loadings). It seems that the main composition of assemblages (PC3) is quite similar to PC1 and does not show more information about the bottom conditions. Thus, we only use PC1 and PC2 in the manuscript to recognize the three assemblages.

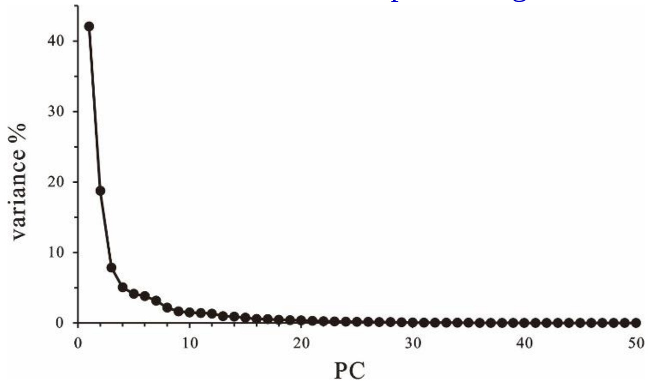

Figure: the variance of total PCs for core MD77-191

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.
Answer: We agree that the dominant species (*B. aculeata*, *S. bulloides* and *H. elegans*) make a significant contribution for these three assemblages, respectively. We recognized three benthic assemblages based on the positive and negative loadings of different PCs. Despite the loading values of *B. manginata*, *C. wuellerstorfi* and *G. subglobosa* are much less compared with the dominant species (*B. aculeata*, *S. bulloides and H. elegans*), both these lower loadings species are environmental sensitive species, associated with different bottom water conditions (e.g., Corliss et al., 1986; Schmiedl et al., 1998; Almogi-Labin et al., 2000). Thus, it seems reliable to use these benthic species for the interpretation of assemblages. In addition, we have plotted the PC1 and PC2 records together in Figure 3, so we prefer not to add an extra figure about the bi-plot for PC 1 and PC 2 in the manuscript.

L. 249: "aragonite": change into "argonitic"
Answer: It has been done. Please see line 259.

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.
Answer: We agree with the reviewer that the description of Cd_w records is similar with the Cd/Ca ratios, so we modified and removed some sentences in the section 5.1 about the discussion of intermediate water Cd_w results from the Northern Indian Ocean to avoid the duplication. We are now providing the following descriptions in the manuscript (lines 274-322):

The intermediate Cd_w results based on the *H. elegans* Cd/Ca values of core MD77-191, range from 0.5 to 3.1 nmol/kg since 17 cal kyr BP (Fig. 4a), with a core top value of 0.80 nmol/kg in agreement with the estimated intermediate water depth modern Cd_w (~0.83 nmol/kg) in the northern Indian Ocean (Boyle et al., 1995). The intermediate Cd_w was also calculated from calcite benthic species *C. pachyderma*, *U. peregrina* and *Globobulimina* spp. from core MD77-191, with values ranging between 0.53-1.48 µmol/mol, 0.52-1.04 µmol/mol and 0.26-0.65 µmol/mol, respectively (Fig. 4a). The Cd_w values of *C. pachyderma* and *U. peregrina* are within the same range. However, the *H. elegans* Cd_w values are higher than those from the two calcite species, especially during the Late Holocene. Moreover, the core top data of *C. pachyderma* and *U. peregrina* are also lower (~ 0.7 and 0.69 nmol/kg, respectively) than the modern estimated Cd_w data (~ 0.83 nmol/kg) in the northern Indian Ocean (Boyle et al., 1995) (Fig. 4a). These depleted Cd_w values may be related to the benthic foraminiferal microhabitat effect; indeed, *U. peregrina* is known to be strictly a shallow infaunal species, as well as *C. pachyderma* (Fontanier et al., 2002), differing from strictly epifaunal taxa, such as *Cibicidoides wuellerstorfi* (Mackensen et al., 1993).

Besides, the deep infaunal *Globobulimina* spp. Cd_w displays relatively much lower values and does not exhibit strong variations compared to the other species investigated in this study, displaying a general increasing trend from the last deglaciation to the Holocene. As *Globobulimina* spp. correspond to deep benthic infaunal species, this result may indicate a stable nutrient content of pore water, as compared to other benthic taxa associated with bottom water (Fig. 4a). Thus, when tracking past changes in the bottom water Cd_w concentrations, the use of a strictly epifaunal species living at the water-sediment interface such as *H. elegans* appears to be more robust than using endofaunal species that live in contact with pore water.

Relative variations in the Cd_w obtained from *C. pachyderma* and *U. peregrina* are in good agreement with the records obtained on *H. elegans*. Variations of *H. elegans* Cd_w during the last deglaciation indicate a decrease of about ~0.6 nmol/kg in the HS1 and YD periods, with a slight increase (0.9 nmol/kg) during the warm B-A. Cd_w results from core MD77-191 indicate a shift from the last deglaciation (~0.7 nmol/kg) to the late Holocene (~1.59 nmol/kg). During the Holocene, the Cd_w records display relatively low values of around 0.9 nmol/kg in the 10-6 cal kyr BP time interval, and show a major shift at around 6.4 cal kyr BP with values rising up to 3.1 nmol/kg.

The reviewer also suggests that Figure 2 might be moved to the supplementary information, but we do not fully agree with this comment. Indeed, comparing the *Globigerinoides ruber* $\delta^{18}O$ records of cores MD77-191and MD77-176 with GISP2 Greenland ice core $\delta^{18}O$ signal (Stuiver and Grootes, 2000) in Figure 2 allows defining the key time intervals we focus, i.e. the Heinrich Stadial 1, the Younger Dryas, the Bølling-Allerød events and the EHCO. We think that it helps to clearly describe the results (Cd/Ca and benthic assemblages), as well as the following discussion. There, we prefer to use Figure 2 here rather than move it to the supplement.

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

Answer: We improved figure 4 with this common. Please see new Figure 4.

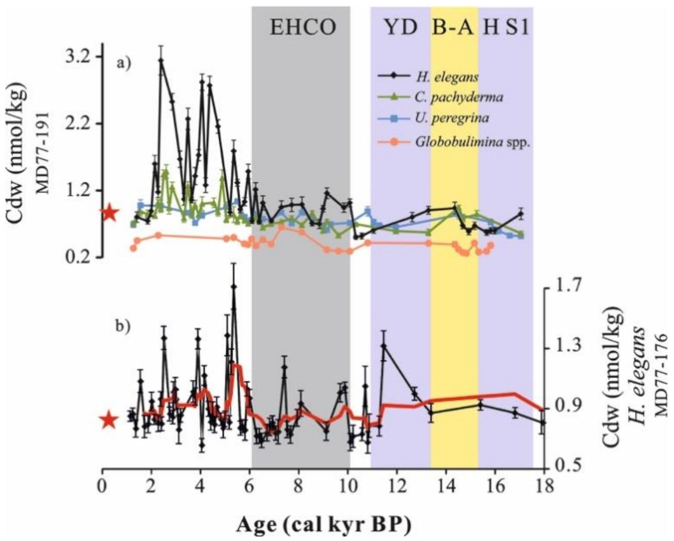

Fig. 4. (a) Cd_w records calculated based on the Cd/Ca of benthic foraminifera *Hoeglundina elegans* (black), *Cibicidoides pachyderma* (green), *Uvigerina peregrina* (blue), and *Globobulimina* spp. (orange) obtained from core MD77-191, (b) Cd_w record from core MD77-176 reconstructed using *H. elegans* Cd/Ca, the red line is the smoothed curves using a five-point average. The red stars represent the modern Cd_w (~0.83 nmol/kg) in the northern Indian Ocean (Boyle et al., 1995). The color shaded intervals and abbreviations are the same as in Figure 2.

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).

Answer: We provide new figures with a common scale for y-axes. Please see new Figure 2. The new Figure 4 is already shown in the reply for the above question "L. 264".

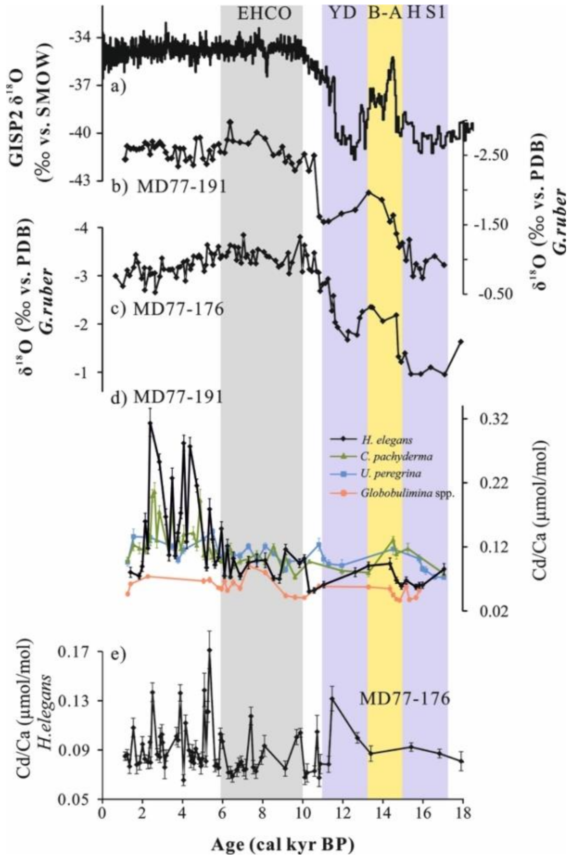

Fig. 2. (a) GISP2 Greenland ice core $\delta^{18}O$ signal (Stuiver and Grootes, 2000). (b)-(c) *Globigerinoides ruber* $\delta^{18}O$ records of cores MD77-191and MD77-176, respectively (Marzin et al., 2013; Ma et al., 2020). (d) Cd/Ca records of the benthic foraminifera *Hoeglundina elegans* (black), *Cibicidoides pachyderma* (green), *Uvigerina peregrina* (blue), and *Globobulimina* spp. (orange) obtained from core MD77-191; (e) Cd/Ca records of the benthic foraminifera *H. elegans* from core MD77-176. EHCO for Early

Holocene Climate Optimum, YD for Younger Dryas, B-A for Bølling-Allerød and HS1 for Heinrich stadial 1.

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

Answer: Corrected. Please see new Figure 4 in the reply for question "L. 264: please add the modern Cdw to figure 4 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.

Answer: Please refer to the reply for question "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?"

Indeed, we indicated in the original manuscript in lines 270-277 (revised version, lines 285-289), which discussed deep infaunal *Globobulimina* spp. Cd_w results. Lines 286-293 (revised version, lines 274-284), we compared Cd_w obtained from *C. pachyderma* and *U. peregrina* with records obtained on *H. elegans*, the depleted Cd_w values of calcite infaunal species may be related to the microhabitat effect, thus *H. elegans* appears to be more robust than using endofaunal species that live in contact with pore water. Although we modified the sentences and structure of the discussion about intermediate water Cd_w results in the section 5.1 to avoid the duplication, we still keep the comparison of these four benthic species Cd_w. Please refer to reply for question "L. 262: The paragraphs discussing Cd_w 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. 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;

Answer: As mentioned before (please refer to question Line 228), the total variance of PC1 (42%) and PC2 (19%) could explain 61% of the total variance, and for other PCs, the total variations are much less. Besides, the main composition of the rest PCs negative or positive loadings is dominated by the same benthic species as these recognized three assemblages (such as based on PC3, 8% total variation, the largest one among the rest PCs), it is difficult to glean more additional information from this regarding bottom conditions. Thus, it seems reliable that we recognize three assemblages in this paper.

2) Why do the authors start with Assemblage 3 not in the numerical order?

Answer: We interpreted our records in time order from the last deglaciation to Holocene.

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.
Answer: The two first-ranked principal components are selected to obtain species associations with demands to specific environmental conditions. Based on the positive and negative loadings of PC1, we recognized benthic assemblages 1 and 2, respectively. In addition, assemblage 3 was identified by the positive loadings of PC2. And then we explored to discuss the bottom water environmental condition changes, combining different major individual benthic taxa of the assemblages, thus, the abundance of respective assemblages may be not suitable as the environmental indicator. Therefore, we prefer to keep the figures in their original version.

L. 351: better write "Assemblage 1" instead of "fauna 1"
Answer: Corrected. Please see line 384.

L. 353. "depleted *Globigerina bulloides* abundances" – replace by "low *G. bulloides* abundances"
Answer: It has been done. Please see line 386.

L. 370: please refer more often to the respective Figures.
Answer: We have added the reference figures 3 and S2 in the revised version. Please see line 412.

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.
Answer: We agree with the reviewer that the Cd_w values are constantly high during 5.2-2.4 cal kyr BP, especially compared with the early Holocene. Thus, we have corrected this description in the revised version (line 440) as:
"reach a maximum during the late Holocene".

L. 405-406: the relation of stratification and PP should be discussed in more detail (see my general remark).
Answer: We used this comment to improve the discussion of the manuscript. We are now providing the following explanation in the manuscript (lines 447-451):
However, the distribution of chlorophyll in surface water of the western BoB suggests a low annual productivity, indicating that the BoB is not significantly influenced by the riverine nutrient input (Zhou et al., 2020). Thus, it is likely that this increase in fresh water drove pronounced ocean stratification in the northeast BoB,

which could impede the nutrient transfer from deep layer to the euphotic upper seawater column, and then inducing low productivity.

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

Answer: Bassinot et al. (2011) indicated that the ITCZ location shifted northward when boreal summer insolation reached a maximum in the early Holocene, associated with the enhanced summer monsoon wind intensity and a decrease in the Ekman pumping in the southern tip of India. This configuration then induced a decrease in surface productivity in the southeastern Arabian Sea. We added the summer insolation variations in the modified Figure 6, please see the new Figure 6.

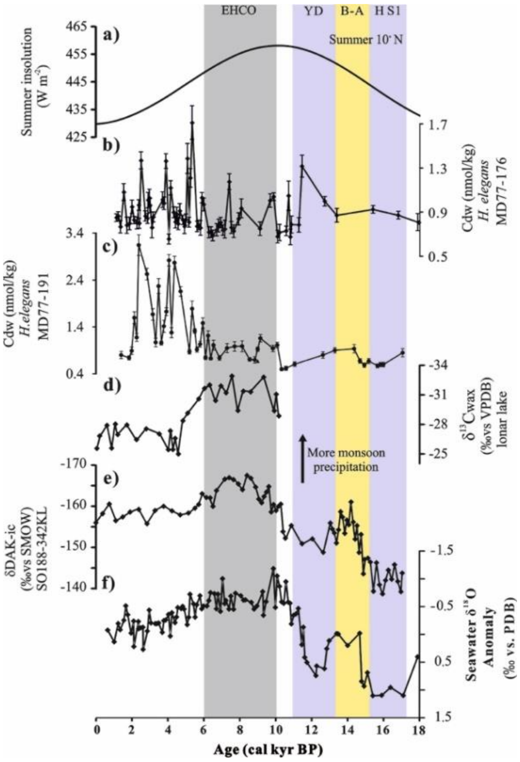

Fig. 6. (a) The solar insolation at 10 °N in summer (Laskar et al., 2004). (b) and (c) intermediate Cd_w calculated from *H. elegans* obtained from MD77-176 and MD77-191, respectively. (d) Lonar Lake $\delta^{13}C_{wax}$ record (Sarkar et al., 2015). (e) $\delta D_{Alk\text{-}ic}$ record from core SO188-342KL (Contreras-Rosales et al., 2014). (f) Seawater $\delta^{18}O$ anomaly obtained from MD77-176 (Marzin et al., 2013). The color shaded intervals and abbreviations are the same as in Figure 2.

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

Answer: Corrected. Please see line 500.

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.

Answer: We agree with the reviewer that these sentences seem to be odd here and removed them in the revised version. Please see lines 505-506.

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.

Answer: We agree with the reviewer and correct these sentences in the revised version. We also fully agree with the reviewer that the C_{org} could depend on PP and/or preservation, this has been also indicated in the revised manuscript (lines 391-393). In this study, we mainly focus on the *G. bulloides* abundance to reflect the paleoproductivity in this region. The modifications of revised lines 462 etc are explained in Reply the second general comment). Please see lines 500-517.

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

Answer: We have corrected this sentence in the revised manuscript (line 535-540) to be:

Thus, as the benthic $\delta^{13}C$ values collected from the north Indian Ocean could better constrain the influence of AAIW in the two studied cores (Naqvi et al., 1994; Jung et al., 2009; Ma et al, 2019; 2020), we can also compare the range values of AAIW Cd_w from both studied cores with data from Atlantic and Pacific Oceans at intermediate water depth during the HS1 and YD (Cd_w, 0.3-0.9 nmol/kg; Umling et al., 2018; Valley et al., 2017).

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

Answer: Corrected. Please see line 549.

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?

Answer: Unfortunately, we have no other productivity indicators records available from core MD77-191 (Arabian Sea).

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)

Answer: For figure 1, we have put the “Arabian Sea” and “BoB” names in the right place. And for these two figures, there are more information should be shown clearly, such as the general surface circulation and primary productivity distribution of the northern Indian Ocean. If we move the “Arabian Sea” and “BoB” names next to the core name, these two figures seems to be “confused”, so we prefer to keep the core name in its original version.

Fig. 5: you might add here the benthic $\delta^{13}C$ records of MD77-191 and -176 used to discussed water mass variability (cf. Fig. S3)

Answer: We do not fully agree with this comment since for Fig. 5, in order to examine the relationships between primary productivity and intermediate Cd_w records of the studied cores, we compare MD77-191 Cd_w results with different records associated to surface productivity from the same area (southeastern Arabian Sea). The similarity of the benthic $\delta^{13}C$-increases reflects the northward expansion of AAIW during the last deglaciation in the north Indian Ocean and Pacific Ocean (Pahnke and Zahn, 2005; Jung et al., 2009; Ma et al., 2019, 2020), the detailed interpretation based on the benthic $\delta^{13}C$ records (including cores MD77-191 and MD77-176) have been discussed in these previous papers, thus we prefer to use Fig. S3 as the supplementary information to interpret the changes of intermediate water masses.

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

Answer: Corrected. Please see new Figure S1.

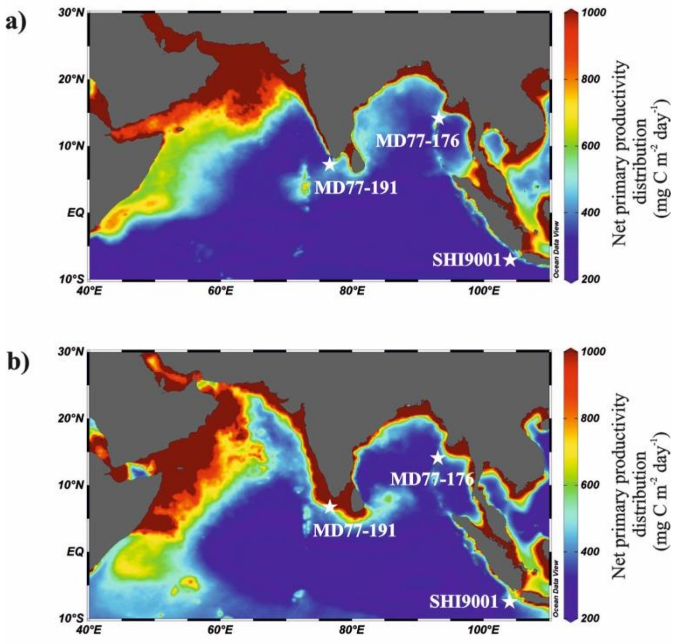

Fig. S1. a) – b) The Net primary productivity distribution in the Northern Indian Ocean during January and July, respectively. Maps based on MODIS chlorophyll-a, SST, PAR satellite data, using the standard vertically Generalized Production Model (VGPM) (Behrenfeld and Falkowski, 1997) as the standard algorithm.