Response to reviewer 3

For easier reading, we have reproduced the reviewer's comments (in black) and give our responses in blue.

I am reviewing a manuscript by Marzin et al. on "Glacial fluctuations of the Indian monsoon and the relationship with North Atlantic abrupt climate change". The study consists of two separate parts: Part one presents a proxy record from a sediment core in the Bay of Bengal covering the last 40,000 years. The authors interpret this record in terms of hydrological changes in the Asian monsoon region. They further conclude that the variability in their record closely follows the D-O type variability of e.g. Greenland ice core records. In the second part, modelling studies are performed with the aim to investigate the relationship between changes in the North Atlantic and changes in the Asian monsoon at millennial timescales.
In this review I will focus on the first part and trust that another reviewer with different expertise will judge the modelling study.

General comment:
The topic of the paper is an interesting and important one and both aspects - additional data and modelling - are needed to understand the origin of millennial scale variability in proxy records from the Asian monsoon region. The authors claim that the presented record is the first record that directly reconstructs variations in the hydrological cycle of the Asian monsoon region at these timescales (page 6271, line 21). If that is the case, then this would indeed be a key record. However, in my opinion the conclusions drawn from the proxy record are in my view unfortunately not well justified in the current version of the manuscript. I think the authors should be a bit more critical with the dataset and try not to exaggerate ("remarkable similarity', "closely correlate" etc: : :.) the presence of potential correlations with other paleoclimate records. The authors quickly jump to the conclusion that the presented record shows clear D-O type variability. I understand that this is a reasonable working hypothesis and probably an expected outcome given that some other studies have claimed such a connection previously. However, I simply cannot not be convinced that there is a good correlation in this case (and that even though the age model is partially based on direct tuning to Greenland. In that sense the argumentation is circular).
Firstly, I suggest to work with an entirely independent age model that is based on radiocarbon dates only (the authors have quite a few of them). Then they can plot the record on this age model against Greenland and then indicate which wiggle in the record could correspond to which D-O event in Greenland: : : Even with the present tuned age model, one would probably not get a significant correlation between Greenland and the Bay of Bengal record at the millennial scale. For such an exercise, the glacial-interglacial variability has to be removed first (high-pass filter). In my opinion one will have to conclude that it is hardly possible to come up with a perfect one-to-one correlation, between the two records. Disagreements should be highlighted and critically discussed. What are the reasons for differences between Greenland and the Bay of Bengal record?

We have modified figure 3 to display the record of the Bay of Bengal seawater $\delta^{18}O$ plotted vs. the age model obtained from radiocarbon ages (transformed into calendar ages using the relationship from Fairbanks et al., 2005). This figure also shows the detail of the correlations that we made using the GISP record and that we used to establish the final age model of our record. The glacial-interglacial variability linked to changes in the mean $\delta^{18}O$ of the ocean was subtracted in the Bay of Bengal seawater $\delta^{18}O$ anomaly record. Only local changes, due to freshwater budget variations, are expressed in the presented record.

As suggested by the reviewer, we modified the text to be less affirmative about the "good correlation" between Bengal Bay and Greenland records. The numerical modelling experiments were indeed performed to test this hypothesis.

1. Is it because of a large uncertainty in the used proxy record?
Is the temperature from the foram assemblage valid for the G.ruber based d18O record (how much uncertainty does this add)?

In the estimation of the seawater $\delta^{18}O$ anomaly, we supposed that there were no changes in the SST of the Bay of Bengal during the last 40 000 yrs. Thus we did not take into account the development season of G. ruber. Following this assumption, we consider that the $\delta^{18}O$ record of G. ruber in the core MD77-176 is controlled only by changes in the mean $\delta^{18}O$ of the ocean *(Global signl)* and in the freshwater budget of the Northern Bengal Bay *(local effect).*

When does G.ruber bloom in this region? Does it really record the peak summer monsoon season? Normally, planktonic forms avoid low-salinity conditions (how much uncertainty does this add?).

Two foraminifer development seasons are found today in the Bay of Bengal. The highest foraminiferal fluxes recorded in sediment traps occur during each of the two monsoons. G. ruber productivity is however reduced in the Northern Bengal Bay during the summer monsoon because of the strong surface salinity lowering (Guptha et al., 1997). Continental runoff increases during summer when precipitation associated to the SW monsoon is higher.

It is reasonable to suppose that, in the Northern Bengal Bay, the G. ruber population development is dominant during the winter monsoon (by comparison to that developed during summer) in the modern foraminiferal assemblages because of the reduced salinity in the northern Bengal Bay during summer monsoon.

As for past conditions, the surface salinity of the Bengal Bay is found to be lower during interglacial intervals than during glacials. We can therefore assume that when salinity increased in the Northern Bengal Bay, the contribution of G. ruber developed during summer increases with respect to that of the winter monsoon. Summer monsoon changes in glacial conditions should therefore be recorded in the $\delta^{18}O$ of G. ruber with a better sensitivity that during modern conditions, a period during which freshwater injection in the Bay of Bengal may be fully detected in the $\delta^{18}O$ records of G. ruber.

This information has been added at the end of Section 2.1.

Is the d18O-salinity relation constant over these timescales (how uncertainty does this assumption add)? Most paleocenographers would be very careful to interpret d18Osw simply as salinity signal (see paper by Rohling in Paleoceanography a few years ago).

The paleoclimatic record presented here is the $\delta^{18}O$ anomaly of surface water and not the salinity. We are aware that the slope of the $\delta^{18}O$-salinity relationship displays spatiotemporal changes at seasonal and annual scales and depends mainly on the runoff effect (Singh et al., 2010). If we attempted in certain passages of the paper to convert isotopic anomalies in salinity change, it was only in order to facilitate the reader's appreciation of the magnitude of changes in terms of salinity. As the slopes of the relationship vary greatly, our estimates of past changes in salinity are expressed as ranges.

I would like to mention here as well, that the method part is incomplete. How were the isotopes measured (instrument?). What is the internal and external reproducibility? Error bars?

This part has been completed.

2. Is it because large uncertainty in the age model? Given the large number of C14 dates, this is probably not the main source of error.

3. Or as alternative: Does the hydrological cycle not follow D-O variability? Are there other, equally important, unknown forcing factors at millennial timescales??

[reponse to points 2 and 3]
We agree with the reviewer that the relationship between the events recorded in Greenland and those from our record from the Bay of Bengal is not a one-to-one relationship. This is precisely what led us to consider modelling experiments to further test this relationship suggested by the data and to offer an explanation for it which would strengthen the conclusion drawn from the data alone. This approach is now hopefully better explained in the new version of the manuscript.

More detailed comments:
page 6271, line 28: "low surface salinity tongue", is that a seasonal feature? What is the seasonal variability in salinity at the core position? and which part of the season does G.ruber reflect in that region of the ocean?

Salinities at the core location are lower than those of the open ocean throughout the year. The question about the impact of the seasonal variations of salinity on the development of G. ruber has been answered above and is discussed at the end of Section 2.1.

page 6274, line 23: mg/Ca in which species? G.ruber? If that's the case, would this not demonstrate that the assemblage temperatures are not a good representation of the temperatures that are recorded by G.ruber. This could be e.g. due to differences in the recorded seasonal range and/or habitat differences. Therefore, the last sentence on page 6274 is not well justified in my opinion.

In the literature, Mg/Ca measurements in the Bay of Bengal cores were performed in G. ruber but Mg/Ca changes also strongly depend on parameters other than temperature, noticeably salinity and carbonate ion content (Mathien-Blard, E., & Bassinot, F. (2009). Salinity bias on the foraminifera Mg/Ca thermometry: Correction procedure and implications for past ocean hydrographic reconstructions. Geochemistry Geophysics Geosystems, 10(12). doi:10.1029/2008GC002353). In addition, we note that the Mg/Ca SSTs record obtained in the Northern Bengal Bay by Rashid et al (2007) display no significant changes within the glacial time. This indicates that the millennial variations in G. ruber $\delta^{18}O$ values are not generated by SST changes.

It would be useful to show these datasets from Rashid et al. and Kudrass et al. in a comparison figure as well.

As our SST record does not displays significant changes during the last 40 kyrs, we do not see the need to add a figure for comparison with the results of other studies. But we do cite these studies.

page 6275, line 6: monospecific (which species?)

Added in the text.

page 6275: in order to correct the d18O data for sea level changes, the Waelbroeck dataset has been used. However, this record is smoothed and does not show much millennial scale variability. In contrast record from Arz et al (QSR) or Siddall et al. (nature) are higher in resolution and show considerable variations in sea level at the millennial scale. I think that these records would be more suitable for the study by Marzin et al.

The studies from Siddall et al (Nature 2003) and Arz et al (QSR, 2007) indeed reconstruct the sea-level, using oxygen isotope measurements from benthic foraminifera from the Red Sea. Their result is in terms of global sea-level and not global ocean $\delta^{18}O$ which we could use readily. Most sea-level variations during the period studied in our record are less than 5m, except for a 20 to 25 m event around Greenland Interstadial 8. If we retain this maximum sea-level change and make the hypothesis of a change of 1 permil in $\delta^{18}O$ for a change of 120 in global sea-level, then the order of magnitude of the $\delta^{18}O$ change associated with abrupt climatic events in global $\delta^{18}O$ would be of around 0.2 permil. This is much less than the typical variability in our record, which ranges from 0.4 to 0.8 permil. Therefore, the variations we have measured in record MD77-176 cannot be entirely explained by global sea-level changes.

page 6275, line 17: "closely correlate": : :.Is there a significantly better correlation with Greenland than with Antarctica? This question is of particular importance since several authors have argued that e.g. the Chinese speleothem record of Hulu Cave contains substantial part of Southern Hemisphere variability (see Rohling et al in QSR or Caley et al in QSR).

Given the chronological constraints for our record and the fact that there is a one-to-one relationship between the Greenland and the Antarctic millennial events, we believe it would be difficult to distinguish the "contribution" of each hemisphere to the Indian monsoon variations presented here. However, we now mention this possible influence from the southern hemisphere in the discussion of our results.
From the point of view of our modelling work, the hosing experiment we use results in much stronger SST changes in the North and tropical Atlantic than in the Indian or Southern Oceans. We have therefore tested the impact of SST changes in these regions. It would be interesting to test the impact of changes in other regions but unfortunately this is presently no longer possible, since we have changed super computers and the model version used here does not run on this new machine. This idea is therefore presented in our perspectives for future work (which we will have to study with a newer version of the model). We can also note that southern hemisphere events could have an impact on the Indian monsoon via the tropical

Atlantic. Our experiments would agree with this statement, but the link between the southern hemisphere extra-tropics and the tropical Atlantic would have to be explained with new experiments.

page 6276, line11: I don't see this Figure 3b ??
This is a mistake, we meant the bottom graph on Figure 3. The text is now corrected.

lpage 6277, line 13: If these records show a remarkable similarity, than this should be at least demonstrated in a figure.

Figure 3 has been modified and the terms employed to describe it have been modified (see above reponse to points 2 and 3).

page 6277, line 14 to line 25: I recommend to include several references here that come up with alternative interpretations of the mentioned speleothem record (e.g Pausata et al in Nature Geoscience, Clemens et al in Paleoceanography) and the Arabian Sea productivity and OMZ records (Schmittner in nature and Paleoceanography, Ziegler et al in Paleoceanography). These studies show that it may be possible, that D-O variability in the mentioned records is introduced by other mechanisms than summer monsoon intensity, processes such as AMOC influence on nutrient distribution in the oceans and consequences for OMZ intensity in the Arabian Sea or ocean temperatures influencing the isotopic composition of the rainfall and thus influencing the isotopic signatures in cave calcites.

We followed the recommendations and raised ideas of the mentioned references in the discussion.

Minors:
Introduction could have some additional references (e.g. in page 6270, line 26). There is a mistake in line 26 page 6270 "experiments from : : :.. (word missing?) have shown that: : :..
ok, corrected. (there was a reference missing). We have also added references about mechanisms suggested by previous modelling studies in the introduction, following the comments by reviewer 2.

page 6271, line 19: : :"depends" seems to be the wrong word here
is it?