

Interactive comment on “Glacial fluctuations of the Indian monsoon and their relationship with North Atlantic abrupt climate change: new data and climate experiments” by C. Marzin et al.

Anonymous Referee #3

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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

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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?

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 $d_{18}O$ record (how much uncertainty does this add)? 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?). Is the $d_{18}O$ -salinity relation constant over these timescales (how uncertainty does this assumption add)? Most paleocenographers would be very careful to interpret $d_{18}O_{sw}$ simply as salinity signal (see paper by Rohling in *Paleoceanography* a few years ago). 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?

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??

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?

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.

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

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

page 6275: in order to correct the d18O data for sea level changes, the Waehlbroek dataset has been used. However, this record is smoothed and does not show much millennial scale variability. In contrast records 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.

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).

page 6276, line 11: I don't see this Figure 3b ??

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

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

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isotopic signatures in cave calcites

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.

page 6271, line 19. . ."depends" seems to be the wrong word here

Interactive comment on Clim. Past Discuss., 8, 6269, 2012.

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