

Interactive comment on “Anomalously high Arabian Sea productivity conditions during MIS 13” by M. Ziegler et al.

Anonymous Referee #2

Received and published: 12 October 2009

The paper submitted by Ziegler et al. presents a rich set of high-resolution data and a very interesting discussion about the possible cause of the major productivity and carbonate dissolution event that occurred during MIS 13 in the Arabian Sea. The authors propose a scenario that rests upon changes in the Meridional Overturning Circulation.

General comments:

I agree that the lack of a distinct, anomalous event in the Antarctica CH₄ record is indeed a strong argument against a global, wet monsoon anomaly in MIS 13. However, the authors cannot totally dismiss the Mediterranean (sapropel; Rossignol-Strick et al., 1998) and the equatorial Indian Ocean (Bassinot et al., 1994) d₁₈O records, which both suggest enhanced precipitations during MIS 13 (and not MIS14 as indicated by Ziegler et al. in the manuscript). The lack of a distinct planktonic d₁₈O spike in core MD04-

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2881, which is located in the North of the Arabian Sea, may not be really conclusive by itself regarding the Indian Monsoon precipitation history. Most of the large river runoffs are directed towards the Bay of Bengal (resulting in the large salinity gradient between the Arabian Sea and the Bay of Bengal). Thus, it is likely that contrasted records of wet Indian monsoon intensity changes should primarily be looked for in sediments from within the Bay of Bengal (or close to it), not in the Arabian Sea.

In addition, in Ziegler et al's manuscript, past summer monsoon intensity changes are deduced from changes in productivity and dissolution, which are tightly linked to the activity of seasonal-upwellings at orbital time-scales. Thus, there is some kind of a shortcut in the author's reasoning when they infer past changes in summer monsoon precipitation and discuss them with respect for instance, to our knowledge of CH₄ evolution. Looked at face value, their core MD04-2881 data are primarily dependent upon wind forcing past variability, not precipitation. This may introduce a tricky complication since a recent work suggests that, in the western Indian Ocean and the Arabian Sea, there could exist a decoupling between orbitally-related changes in precipitation and surface wind tension (Malaize et al., 2006 – G-cubed, Q12N08, doi:10.1029/2006GC001353).

I suggest, therefore, that the authors address carefully these two questions in a revised version of their manuscript: * 1/ are planktonic d18O data in the Arabian Sea good recorders of past changes in Indian monsoon precipitation history and can we confidently rule out the possibility of an anomalous wet-monsoon event in MIS 13 based on Core MD04-2881 d18O record? * 2/ can we confidently use Arabian Sea records of paleo-productivity and -dissolution (which are ultimately related to wind forcing at the precession time scale) to infer past changes in summer monsoon precipitation (and compare them, for instance, to the high-latitude, Antarctica CH₄ ice record)?

Discussing these elements will not rule out the MOC hypothesis put forward by the authors to explain the MIS13 productivity anomaly. Yet, it could lead to a more subtle conclusion regarding low-latitude precipitation history and the possible "odd monsoon event" in MIS13.

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Specific comments:

1/ Page 3 : "This period of extensive dissolution in the deep sea is probably not caused by enhanced greenhouse forcing. . .". I would suggest to replace "caused by" (which gives a wrong sense of direct causality. It is not the atmosphere that drives the ocean, but the opposite) by "related" or "associated", ..

2/ Page 3 : The Bassinot et al. 1994 reference to the Mid-Brunhes dissolution interval is not the same as the reference to the MIS14, anomalous d18O event. The former is : Bassinot, F., Beaufort, L., Vincent, E., Labeyrie, L., Rostek, F., Müller, P.J., Quidelleur, X., and Lancelot, Y. (1994). Coarse fraction fluctuations in pelagic carbonate sediments from the tropical Indian Ocean: a 1,500 kyr record of carbonate dissolution, *Paleoceanography*, 9(4), 579-600. Thus references to Bassinot et al. 1994 papers should be labeled 1994a and 1994b in the text (and the corresponding 1994b reference added to the bibliography).

3/ Page 6 : Change "PeeDeeBeleminte" to "PeeDeeBelemnite".

4/ Page 10 : the authors should specify in the text that the Ti/Al record is from discrete measurements. This is indicated in the figure 10 caption, but because the authors had carefully underlined in the "methods" that normalization to Al cannot be done with XRF core scanner data, it should be plain clear to the reader in the "result" chapter that the authors discuss the discrete sample Ti/Al record.

5/ Page 15 : In the sentence "..suggestion that the climates of both hemispheres are unusual asymmetric during MIS13", change "unusual" by "unusually".

I look forward to seeing this paper published in *Climate of the Past*, once the corrections and comments have been properly taken into account by the authors.

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