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Climate of the Past Discussions

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

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Below we include a reply to each of the reviewer's comments. We are grateful for the insightful comments regarding this manuscript, and feel that the manuscript has been improved and now address the points which were raised. In cases where we respectfully disagree with the referees we explained our reasoning.

The main criticism on the manuscript focuses on the question whether productivity is a reliable, quantitative, monsoon indicator or not. Different proxy records basically suggest a different intensity of the monsoon during MIS13. This manuscript discusses the observed differences in these proxy records, both in terms of monsoon intensity and additional processes involved.

1st Review: Steven Clemens

Specific comments

Clemens: 1. page 1991, lines 10-15. presents the loess, equatorial Indian, and Mediterranean evidence for strengthened MIS 13 summer monsoons and immediately dismisses these as having occurred during MIS 14 and therefore of different origin. This is not the case as illustrated in the attached figures 1 and 2; both anomalies are within MIS 13 and thus, difficult to dismiss.

Answer: In case of the sapropel A in the Mediterranean we refer to the original publication by Rossignol-Strick and co-workers (1998), in which they conclude on the timing of Sa: "Sapropel A is seen from 2,301 to 2,295 cm, and shows a very depleted δ18O value, as in all the other sapropels. Figure 2a, b shows its detailed chronological and isotopic setting between interglacial stages 13 and 15." More recently, the timing of the Sa sapropel was evaluated by Lourens (2004), showing that it occurs within MIS 14, but $\sim$20,000 years earlier as originally proposed. For the equatorial Indian Ocean isotope spike we refer later in the text to an explanation, based on the original paper by Bassinot. "We note also that, Bassinot et al. (1994) suggested that the isotope peaks in the equatorial Indian Ocean record are potentially related to an autochthonous sediment lens."

We do not dismiss, however, that the thick soil horizon in the Chinese loess record occurred in MIS 13. This has now been clarified in the revised manuscript.

Clemens: 2. 1994 lines 3-5. Authors might consider a discussion of closed-sum issues surrounding use of raw counts.

Answer: We did not normalized XRF Ba-counts using Al-counts, because results for the light element Al are not reliable quantitatively using XRF scanning. A comparison of our Ba count-profile with a low-resolution Ba/Al profile based on conventional XRF measurements using discrete samples shows a perfect match (Figure 3d). This perfect

match is why we conclude that closed-sum issues did not influence our record, in this particular case. We clarified this point in the revised version.

Clemens: 3. 2000 lines 21-24. The Kutzbach 1981 model result is interpreted as '...indicating that tropical monsoons respond primarily to changes in Northern Hemisphere summer insolation on orbital time scales'. This is a significant misinterpretation of this reference. The only forcing present in the Kutzbach 1981 model run was insolation forcing. Thus, it is not surprising that the model monsoon responded primarily to insolation forcing. Kutzbach's elegant experiment was designed to see IF insolation changes at the orbital scale were sufficient to drive climate change. This insolation-only experiment cannot be interpreted to indicate that insolation is the primary driver at orbital time scales nor what the phase of the summer monsoon is relative to insolation forcing. Time dependent experiments using realistic global ice volume and other lower boundary conditions (e.g. greenhouse gasses) are necessary in this regard.

Answer: We agree that the early Kutzbach studies should be regarded as sensitivity test to orbital forcing, rather than full climate simulations. This has been changed in the text. However, here we present the outcome of new transient climate modeling experiments, which were performed with a climate model of intermediate complexity (CLIMBER-2). This model study now incorporates, in addition to changes in orbital parameters, glacial-interglacial ice-volume variability as well as changes in the concentration of greenhouses gases of the last 650, 000 years. Details on this model are presented in separate papers (Tuenter et al., 2003, Ziegler et al., in review, Weber and Tuenter, in prep.). This climate modeling approach suggests that summer monsoon intensity changed in-phase with changes in northern hemisphere summer insolation (precession minima). Evidently, these results are in line with the early Kutzbach results concerning the impact of insolation.

Clemens: 4. 2001 lines 4-8. This text indicates that manuscript figure 4 shows that the summer monsoon indicators (Ba and shell normalized weight; SNW) are consistent with the methane record and the CLIMBER-2 monsoon precipitation results from

which they '..conclude that the productivity changes in the Arabian Sea primarily reflect changes in summer monsoon upwelling: : :'. Here a significant contradiction arises. The CH4 record is taken as a paleo indicator of summer monsoon strength as is the CLIMBER-2 model result. The contradiction is that maxima in the Ba (and SNW) summer monsoon proxy peaks are clearly out of phase with maxima in the monsoon model result and CH4 (attached figure 3). The timing of Ba and SNW maxima are consistent with the timing observed in a wide variety of summer monsoon proxies from other cores in the Arabian Sea indicating the possibility that the model results and CH4 maxima do not reflect the timing of summer monsoon maxima. This is not necessarily surprising for CH4 given that it has a great number of high- and low-latitude sources that are not driven by monsoon processes at orbital timescales. The CLIMBER-2 model is not sufficiently described to assess why it might not have the same timing as the proxy data. In any case, this contradiction requires attention.

Answer: We note that our wording lead to a misunderstanding of our interpretation of the variations observed in the Ba record and also in the SNW of G. ruber In the revised version of the manuscript we clarified that we take the CLIMBER modeling results, the EPICA methane record as well as the Chinese speleothem (Sanbao, Hulu Cave compilation) as the indicators for past monsoon strength. These independent records all exhibit closely-related precession phases (i.e. between 0 and $\sim$3,000 years for the last 225,000 years: Ziegler et al., in review). Since the productivity records from the Arabian Sea show a contrasting (i.e. 6,000-8,000 years) phase relation with precession, we argue that they primarily responded to another forcing mechanism. We suggest (in contrast to earlier studies) that not wind strength but nutrient availability linked to the intensity of the global conveyor circulation controlled orbital-scale productivity changes. We furthermore argue that also the Loess records, which indicate anomalous conditions during MIS 13, are probably not related to changes in summer monsoon precipitation but instead by to mild winter conditions.

The text has been rephrased accordingly: "A modelling study showed that increased

NADW formation affects primary productivity and OMZ intensity in the Arabian Sea through increased nutrient availability on millennial time scales (Schmittner et al., 2007). In a separate study, we argued that the orbitally-induced primary productivity changes in the Arabian Sea are also very sensitive to the global ocean circulation rather than only summer monsoon intensity, causing therefore a much longer precession phase-lag (Ziegler et al., under review). In analogy, we propose that the productivity peak and associated anomalous dissolution event during MIS 13 relates to increased Atlantic overturning circulation around TVI. At the same time increased heat transport to high northern latitudes might have caused the exceptionally mild winter conditions in Eurasia. Denton and co-workers (2005) suggested that the winter climate was much more sensitive to past changes in Atlantic meridional overturning, due to sea-ice formation. Accordingly, intensified AMO resulted in mild winter conditions, facilitating soil formation on the central CLP. This implies that both Arabian Sea productivity and CLP soil formation was effectively decoupled from Asian summer monsoon intensity during MIS 13." Clemens: Technical corrections 1992 line 1 – 'recovered' as opposed to 'drilled'?

Answer: This has been changed accordingly.

2. Review: Anonymous

General comments: Anonymous: 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).

Answer: As we explained above, it was the original study by Rossignol-Strick and co-workers (1998) that timed sapropel Sa between interglacial stages 13 and 15. This implies that irrespective of the more recent recalibration at $\sim$20,000 years earlier,

sapropel Sa occurred during stage 14. The oxygen isotope spike in the equatorial Indian was interpreted by Bassinot et al. (1994) not to signify enhanced precipitation, but more likely related to an autochthonous sediment lens. Comparing the Bassinot isotope record with the CLP soil record and the Arabian Sea Ba record, also clearly shows the completely different character of these peaks. Whereas the oxygen isotopic spike appears to be a relatively short lived, but high amplitude event, the other records suggest events of longer duration. This is now explained in more detail in the text.

Anonymous: The lack of a distinct planktonic d18O spike in core MD04-2881, 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).

Answer: We agree with the reviewer that upwelling intensity/productivity in the Arabian Sea and summer monsoon precipitation are decoupled on orbital timescales. This view is also supported by the results of Malaize and co-workers (2006, reference has been added in the revised manuscript). However, Arabian Sea productivity is not necessarily

related to increased wind strength alone. We argue, supported by ocean modeling (Schmittner, 2005; Schmittner et al., 2007), that Arabian Sea primary productivity and OMZ intensity is sensitive to nutrient availability. Ultimately productivity is thus linked to the intensity of Atlantic meridional overturning circulation.

This reviewer is not convinced that the lack of a clear isotope peak in the Arabian Sea signifies a normal rather than an enhanced summer monsoon. His main argument is that freshwater signals (and thus a response in the planktic d18O), because of the riverine discharge, are mainly directed to the Bay of Bengal. However, Malaize and co-workers (2006) actually use reconstructed precipitation based on planktic foraminiferal oxygen isotope values to study phase relations in the Arabian Sea. This implies that, although small scale changes might be recorded better in the Bay of Bengal, major increases in monsoon strength and thus precipitation should still show up in our d18O record. This has now been explained in the text.

Anonymous: 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? *

Answer: The absence of a d18O peak serves primarily as additional argument. It merely shows that previously observed peaks are not basin-wide, in this way arguing against an Indian Ocean wide freshening due to an anomalous strong summer monsoon. As pointed out earlier in the review, low atmospheric methane concentrations speak strongly against an anomalous wet monsoon event in MIS13. Methane records mirror our modeling results, showing no extreme monsoon event during MIS13. Recently this line of reasoning is further substantiated by new results from the Sanbao Cave speleothems. The extended cave record now shows no anomalous isotope sig-

nature during MIS 13, arguing against abnormally high rates of precipitation during MIS13 as well (Hai Cheng personal communication, work in preparation)

Anonymous: 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)?

Answer: We fully agree with the reviewer, that Arabian Sea productivity cannot be linked in a direct way to summer monsoon precipitation on orbital timescales. This is exactly the focus of our paper. We have changed the text to make this clearer, concluding that increased productivity during MIS 13 by itself is not necessarily suggesting intensified monsoonal precipitation.

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", ..

This has been changed accordingly.

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., MuÌĹller, P.J., Qquidelleur, 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).

This has been changed accordingly.

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

This has been changed accordingly.

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.

This has been changed accordingly.

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

This has been changed accordingly.

Interactive comment on Clim. Past Discuss., 5, 1989, 2009.