Clim. Past Discuss.,
https://doi.org/10.5194/cp-2017-75-AC1, 2017

Climate of the Past Discussions

Interactive comment on "Hybrid insolation forcing of Pliocene monsoon dynamics in West Africa" *by* Rony R. Kuechler et al.

Rony R. Kuechler et al.

dupont@uni-bremen.de

Received and published: 24 October 2017

Anonymous Referee #1

COMMENTS / CLARIFICATIONS Section 1.1 (page 3) on the regional climatology is good. I feel that it is missing the direct link to the position of the core site (i.e. is ODP 659 in the northern region where there is only one annual peak of precipitation). I assumed that Figure 1 confirmed the single annual peak of precipitation for the core site, but I then realised that this onshore record is located quite far away (âĹij12°N, 7°W). The position of this field location needs to be on the map of figure 2 to be more explicit in its relationship to the core site, and to confirm that it is also under the same hydrological regime (i.e. not in the zone of the double-annual peak in precipitation).

Printer-friendly version

Discussion paper

The latter point is particularly important to clarify when the text also notes the role of the Sahara in separating the winter/summer monsoon regimes (line 31).

Response. We will add the position of the field location to the map in Figure 2. In addition, we'll add the position in Figure 3 with its present-day precipitation and vegetation maps. The main purpose of figure 1 is to illustrate the relation between amount of rainfall and δD of rain. Its illustration of the modern rainfall regime is of less importance, as we document important differences between the Pliocene and the present.

2. Materials and Methods (1) Although the detailed methods are described elsewhere it would be useful to state here what the composite core depths and/or IODP sample identifiers of the top/bottom samples of the two Pliocene sections are.

Response. We'll add cores and datums to this section. More detailed information can be found at the PANGAEA data base (page 9, line 20).

(2) Two age models are noted (page 4). How different are these in terms of the timings of events? Would the authors have found the same relationships to insolation if they had used the dust model? I ask because tuning to the LR04 stack in the Pliocene can be challenging given the low signal-to-noise in the original data and in the stack itself, which could introduce errors in the absolute age and in turn, affect the strength of the conclusions here.

Response. We acknowledge the critical view on the chosen age model, since we have been aware of this challenge from the beginning of its application in our study. We'll add a small section in the Discussion summarizing the following arguments. First of all, the δ18O record of ODP Site 659 is part of the LR04 stack and thus better matches the LR04 stack, which is necessary for the correction of the δD record for changes in global ice volume (page 7, lines 5-9). However, there are further reasons for why we decided to apply the δ18O-based age model. In general, both age models (dust and δ18O) contain the same orbital frequencies. However, the dust model assumes only precession as the main forcing and accordingly, Pliocene dust peaks are tuned

Printer-friendly version

Discussion paper

to insolation minima. Thus, if using the dust age model, the relationship of δD to insolation would be the same as for the dust to insolation, which assumes precession forcing. Precession is also important when using the alternative age model based on δ18O, but only for periods with large variations in precession, while obliquity has the stronger impact during times of low precession variability. By application of the dust age model forcing by precession would be exaggerated in the δD record, since both records (dust and δD) are well correlated, i.e. arid and humid periods reflected in both records are in accord with each other. Further support for using the δ18O age model provides the comparison of the Pliocene sections with the Last Glacial Cycle, which can be regarded as a reference section for the postulated hybrid insolation forcing. The Last Glacial Cycle has an age model based on δ18O and shows very clearly the two modes of insolation forcing (precession vs. obliquity). These are also apparent during the Pliocene, but less well expressed, attributed to minor ice-albedo feedbacks before the intensification of Northern Hemisphere Glaciation. Having said this, the age models are not very different. We'll prepare a figure that plots the benthic stable oxygen isotopes of ODP Site 659 on both time scales comparing with LR04. We'll provide this figure as supplement (SF 1). It is clear that the adapted time-scale of Clemens (T94R) results in the better correlation with LR04. Additionally, we provide a figure showing the results of REDFIT analyses (Schulz & Mudelsee 2002) for dust % (Tiedemann) and δDwax (this study) illustrating that the differences in time scales have very little influence on the position of the significant power maxima (SF 2). A rerun of the wavelet analysis with the original timescale of Tiedemann (T94) also gives very similar results (not shown).

(3) line 28 says that Pleistocene data was used, but this is the first statement of Pleistocene data thus far (the abstract indicates only Pliocene results). Clarify this (perhaps through addressing point (1) above).

Response. The Pleistocene data have been published (Kuechler et al. 2013). They are shown as a comparison. However, this was not mentioned in the caption of table and figures. We will add the reference. As the paper concerns the Pliocene and not the Pleistocene, the Pleistocene data are not mentioned in the Abstract. To avoid confusion, we'll add in the Abstract that data are compared with those of the Last Glacial Cycle.

Page 5 line 2: strange formatting on the first statement of n-alkane concentrations?

Response. Yes, this error will be corrected into 'between 0.01 and 0.8 μg g$-$1'.

Page 5 line 6: clarify if this is a trend 'down-core' or 'towards present'

Pesponse. The trend started at 3.2 Ma, which should mean 'towards present'. We'll add the words to the sentence.

Page 5 (Results). The authors discuss the changes to stable isotope variability in relation to precession and obliquity. The spectral analysis results are quite complex and a clear message is not obvious: this likely reflects, in part, the short durations of the records as well as the complex climate relationships that the authors discuss later. This complexity also underpins my earlier comment about how different the two age models for this core site could be: would the same or similar results have been found with the alternative model? Although the caption notes the role of the cone of influence, the text doesn't make clear that statistical significance of orbital periods should not be assigned to e.g. the 100 kyr signal in d13C in the earliest MPWP. The figure 5 caption also needs to clarify that the boxes on the lowermost panel are assigned according to the original orbital parameters and not the data. With these clarifications the strengths and the caveats of the data may be more explicit.

Response. The question on the different outcomes by using different age models has been addressed above in (2). We'll add a reference to Laskar et al. (2004) and the website address to the caption of Figure 6.

4. Discussion. (1) Page 7 line 20-21: I found it quite difficult to confirm the statement that almost all recognised sapropels could be correlated with the dD31 maxima. I

appreciate that Fig 6 is already quite detailed, but perhaps putting asterisks to mark the timing of each sapropel onto the dD31 maxima plot could help here?

Response. We will add an asterisk to each maximum associated with a Mediterranean sapropel. It should be noted that the sapropel record is tuned to Northern Hemisphere insolation maxima at 65 °N (see e.g., Emeis et al., 2000). This implies a better correlation of sapropels and δD during times of strong precession variability and thus summer insolation, compared to periods of weaker precession variability.

(2) The discussion of the links to insolation forcing is good, and acknowledges the difficult relationships between the climate records and the expected insolation forcing (Page 7). Would the generation of phase wheels or some other coherency analysis help here to visualise the links between the forcings and the feedbacks? It is possible to look at how phasing evolves through time for different proxy records using coherency analysis in evolutive spectra as shown here (phase wheels would have trouble detecting such changes within these narrow windows of time). Such analysis might offer some strength to the arguments made about forcing-response, as well as making figure 6 easier to digest.

Response. The suggested approach of a coherency analysis is ambitious and challenging. This might be part of future work. We feel that our records do not have the resolution nor the length to calculate the evolution of phase relationships. Considering the development of phasing through time, we should take into account that the average sample resolution of the records is $\sim$4 kyr and that detection of leads or lags between the proposed forcing and the climate proxies is limited by the sample resolution and could yield spurious results.

(3) When the authors originally discussed the age models they noted that the alternative (Tiedemann) tuned the dust record. It would be interesting to comment in the text about why Tiedemann did this and whether his assumptions have been verified or refuted by this new data (e.g. if he considered only precession to be key, what do the

Printer-friendly version

Discussion paper

new findings here say about whether that age model could or should be revised?).

Response. Tiedemann argued that for the period older between 5 and 2.85 Ma the dust record had the better amplitude to tune. Please remember that at that time there was worldwide only one other high resolution δ18Obenthic record (ODP Site 846 from the eastern Pacific) to compare with. Nevertheless, years later Clemens managed to tune on the obliquity signal in the stable oxygen record. The Clemens time-scale shows a better fit with the global stack of Lisiecki and Raymo (note that the stack became available another five years later). These are interesting developments in the history of science. However, we feel that it is beyond the scope of our paper. The general assumptions for the Pliocene dust-tuned age model by Tiedemann et al. (1994) are the dominant influence of low-latitude summer insolation (driven by precession), due to the considerably decreased climate variability at high latitudes before the intensification of Northern Hemisphere Glaciations and that during the Pliocene, high-latitude climate variability had little impact on the low latitudes. This might be considered an apriority. Moreover, tuning of the dust record would introduce some circular reasoning in our argument as the dust record partly depends on precipitation. We'll add a small section in the Discussion about the choice of age model listing the arguments mentioned above (under 2 & 3). The implications of the hybrid forcing concept are mentioned in a condensed form at the end of the Discussion section (page 9, lines 9-10): "These findings caution against a too simplified view on orbital forcing mechanisms of the (tropical) hydrologic cycle and hence, related records." These 'related records' are quite a few, including the dust record by Tiedemann, but also the Mediterranean sapropels. We tried to emphasize the general implication of our study by using the word 'caution'. However, with regard to the question on the revision of the dust-based age model, Clemens (1999) already presented a revised age model based on δ18O.

Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2017-75, 2017.

Stable oxygen isotopes of benthic foraminifera of ODP Site 659
on the original time-scale (T94) and
on the adapted one of Clemens (T94R)
compared to LR 04 (Lisiecki & Raymo 2005)

Kendall's tau correlation	LR 04	T94
T94	0,18	
T94R	0,28	0,20

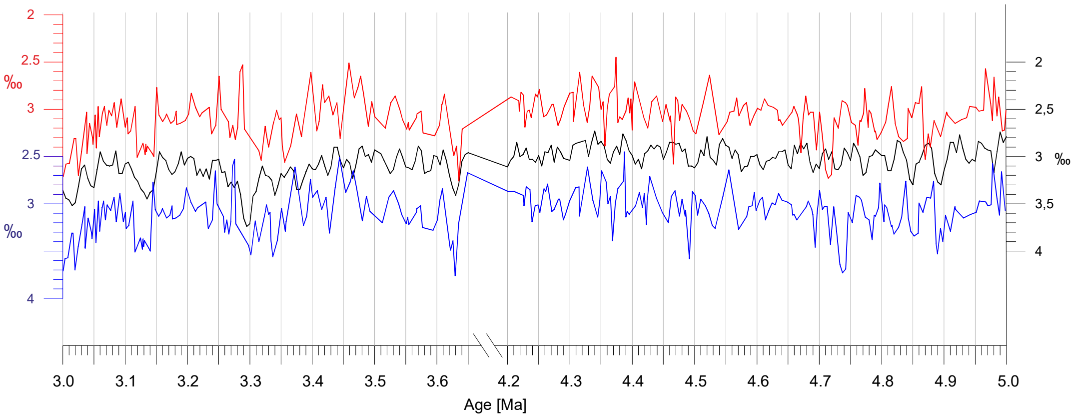

Fig. 1. SF1

Printer-friendly version

Discussion paper

Spectral analysis using REDFIT (Schulz & Mudelsee 2002) comparing the effect of different time-scales –the original dust tuned time-scale T94 in grey (Tiedemann et al. 1994) and the $\delta^{18}O_{benthics}$ based time-scale T94R in black (Clemens 1999)– on the frequency analysis of dust% (Tiedemann 1991) and δD_{31} (this study). The anaylsis is carried out in PAST (Hammer et al. 2001) using one segment and "false-alarm lines" based on parametric approximations (X^2-test). The Mid-Pliocene part runs from 3.62 to 3.00 Ma and the Early Pliocene part from 5.00 to 4.66 Ma.

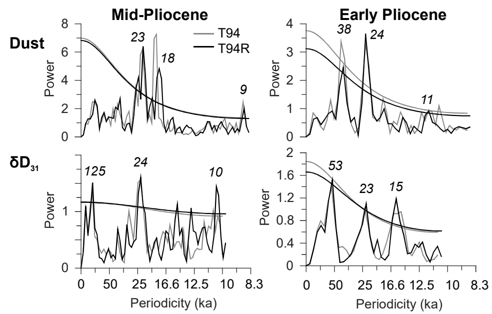

Fig. 2. SF2

Printer-friendly version

Discussion paper