Clim. Past Discuss.,
https://doi.org/10.5194/cp-2017-75-RC1, 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.

Anonymous Referee #1

Received and published: 19 July 2017

Keuchler, Dupont and Schefuss present new results from marine sediment core off west Africa, which record changes to onshore humidity during the relatively warm Pliocene epoch. The authors have analysed the stable carbon and deuterium/hydrogen isotope compositions of leaf waxes for three intervals of Pliocene climate, with an aim of testing the nature of the hydrological response to insolation forcing during a time window when the additional feedbacks imposed by the growth and retreat of large northern hemisphere ice sheets are not at play. The paper is very significant for its presentation of the first records of humidity changes within the Pliocene from western Africa. The results show different patterns relative to the last glacial cycle, and critically, the authors put forward some interesting new potential mechanisms to link subtropical hydrological changes with insolation forcing. The ideas and data outlined here will stimulate further

Printer-friendly version

Discussion paper

research interest in understanding Pliocene circulation changes, but also in determining how and when that system evolved towards the present.

Overall I found the manuscript to be well written and clearly presented. The Introduction is particularly comprehensive and gives both a strong justification for the paper and the relevant literature background to explain the approach. Likewise the opening sections of the Discussion are excellent in assessing some of the potential caveats of the data and its interpretations. The graphics are excellent overall. My main concerns below are minor and are largely for clarification, to ensure that the authors can more clearly demonstrate the strength (or not) of the links between their data and the orbital/insolation forcing.

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 (∼12*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). 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).

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.

(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,

Printer-friendly version

Discussion paper

which could introduce errors in the absolute age and in turn, affect the strength of the conclusions here.

(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).

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

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

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.

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?

(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.

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

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

Printer-friendly version

Discussion paper