

September 10, 2015

Journal: CP

Title: Obliquity forcing of low-latitude climate

Author(s): J.H.C. Bosmans et al.

MS No.: cp-2015-9

MS Type: Research Article

I am reviewer 1, I can agree on the responses to my earlier review, but I still see some things that need to be clarified:

1. lines 60-61: Show somewhere that SITIG fits better to the sapropel record than other indices. This conclusion can not be drawn from Figure 2 right now. Maybe this was in detail already shown somewhere else, then give the reference to this at the end of the sentence.
2. line 95 “obliquity signal on the tropics” change to “obliquity on tropical climate”.
3. lines 93-96: Hazeleger et al 2011 is the reference to EC-Earth, and should be given, when EC-Earth is mentioned as tool first (line 94). You then mention, this was used in the IPCC AR5, so then please give a full reference of the relevant IPCC chapter, in which it was used, or of the whole IPCC AR5.
4. line 98: What do you take from Tuenter et al 2003? the details of the simulation scenario, please say so.
5. line 99-100: When you introduce Tmax and Tmin, please mention once, that “T” is for “tilt”.
6. line 112: Match to the sapropel records. Again, I can from Figure 2 not see any curve that fits better than the other to the sapropels. Some improvement is necessary here, maybe another record, maybe some frequency analysis.
7. line 132: Say BY HOW MUCH the insolation gradient varies, I get 20 W/m² from Fig 1.
8. line 143: paleoclimate records
9. line 155, 159: Precipitation over Sahara and S America and monsoon winds (not shown). Please focus your text on what is shown, so describe the figures you plot here.
10. line 160: include “wind speed change IN BOERAL SUMMER are small”.
11. line 163: change “net precipitation DURING AUSTRAL SUMMER increases”.
12. line 167: Why should that be in agreement with the SITIG mechanism? Was this already written somewhere, if so cite, if not explain.
13. line 173, fig 7 is referred to BEFORE Fig 6, so change order of figures.

14. line 208: The long form if the acronym SITIG is again explained, not necessary.
15. line 222: “Winter (intrahemispheric) insolation gradient does not vary with obliquity”. According to your Figure 1, this statement is wrong. Maybe you should include which points you want to compare here in Figure 1 to guide the reader. Please check and correct, if necessary.
16. line 235: “P” and “T” are never explained, also not what is meant with “P-1/2T” in detail. Does it refer to annual mean or any specific dates?
17. line 242: SITIG is NOT a BOREAL signal, something wrong here.
18. line 244: change “reconsidered” into “rejected”
19. line 246: change “model results” into “analysis presented here”
20. line 287, 289: Do not refer to NOT SHOWN results, so include new figures or reduce discussion.
21. line 308: change “reconsidered” into “rejected”
22. Figure 1: Explain what Tmax and Tmin mean in the caption, obliquity is not even mentioned here.
23. Figure 2: The lower part (sapropels) are discussed in the text to be more in line with SITIG, actually, I do not see any of that. It was said that the weakness of the sapropel signal in S4 and S7 with respect to the others is a key component of the obliquity imprint in the data, and the relation to minima in the insolation indices. If this is really one key message, then please introduce vertical lines on these 2 intervals to guide the eye, but also think about other sapropel records to be shown here.
24. Figure 3: consider a frequency analysis of the sapropels to be included here, maybe also on a longer time series to have a robust signal.
25. Figure 8: Changes in the Hadley cell. Please mention the interpretation of changes in the Hadley cell as given in the discussion also in the caption of the figure. I am not sure the annual mean changes in the Hadley cell (Fig 8c) are important. From the line of argument given I had the feeling, all is said with the description of the changes in subfigures 8a and 8b (JJA vs DJF).