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Interactive comment on "Impact of the Megalake Chad on climate and vegetation during the late Pliocene and the mid-Holocene" *by* C. Contoux et al.

C. Contoux et al.

camille.contoux@lsce.ipsl.fr

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The authors would like to thank the referee for his interest to the paper, and for his relevant comments. We answer the comments below and will integrate these answers in the manuscript.

Page 1370, line 18. "More description is required of the orbital configuration used for the orbital sensitivity simulations. What time during the mid-Pliocene does the maximum summer insolation at 30N occur and how does this compare to other orbital sensitivity simulations that have been run in palaeoclimate studies?"

We agree that this simulation is not detailed enough. The maximum of insolation at

30°N is simultaneous to the maximum of insolation at 65°N and occurs at 3.039 Ma (see figure attached, time in kyr from 3 to 3.3 Ma, brown curve is summer solstice insolation at 65°N, green curve is summer solstice at 30°N). This is equivalent to the Plio_NHmax configuration used in Dolan et al. (2011), although to force the GCM, she used values of orbital parameters corresponding to maximum summer insolation, whereas we used the insolation at summer solstice, hence small differences between our values (3.039 Ma) and her values of orbital parameters (3.037 Ma)(see figure attached, blue curve is mean summer insolation. It is clear on the figure that the maximum summer insolation is after the maximum summer solstice insolation). Nevertheless, only 2000 years separate the two configurations, so the orbital forcing is very similar. We added these sentences in the manuscript: "The maximum of insolation at 30°N is simultaneous to the maximum of insolation at 65°N and occurs at 3.039 Ma. This is very similar to the Plio_NHmax configuration used in Dolan et al. (2011)."

Page 1370, line 22. "How well do the model simulations represent regional sea surface temperatures in the mid-Pliocene? There are no sites from the Gulf of Guinea, but there are a number of sites in the eastern Atlantic from which this could be assessed."

We thank the reviewer for this relevant comment. It is indeed important to assess the quality of these simulations with respect to the data. We added this in the paragraph: "Pliocene SST anomalies in the Eastern Atlantic compares well with some ODP sites data (Dowsett et al. in review). For example site ODP 659, situated off the coast of Mauritania at 18°N, shows an anomaly of +2.26°C, while the simulated anomaly with IPSL-CM5A is 2.3°C. Site ODP 958, situated more northerly at 23°N, compares also well with modelled SST, with a warming anomaly of 2.29°C compared to modelled 2.03°C. Other sites of the Eastern tropical Atlantic (ODP 667 and 661 and DSDP 366, between 4 and 10°N) show no change or a warming of maximum 1°C, whereas the model simulates between 1.5° C to 1.8°C of warming (Dowsett et al., in review). For the North Atlantic, model SST anomalies are several degrees colder than SST estimates from Dowsett et al. (2010), but this is a general feature of all models in the PlioMIP

(Dowsett et al., in review).”

Page 1371, line 15. “It would be good to again assess these mid-Holocene simulations against the available sea surface temperature data.”

We added : “In order to assess the quality of these simulated SSTs, we use the GHOST database from Leduc et al. (2010) to do a model data comparison of SST anomalies at 6k. We take the difference between the value for 6k and the core top to calculate anomalies (as in Hargreaves et al., 2013). Since alkenones and Mg/Ca show opposite trends throughout the Holocene for the Gulf of Guinea and some North Atlantic sites, we decided to use only alkenone data, despite the fact that high latitude alkenone SST records could be biased towards summer months (Leduc et al., 2010; Prahl et al., 2010). For the Gulf of Guinea, alkenone-derived SSTs decribe colder conditions at 6k (-1.9°C) that the model reproduces, although not to the right amplitude (only -0.44°C for the modelled SST anomaly). Off the coast of Mauritania, at 19°N, the modelled anomaly (+0.34°C) correctly reproduces the warmer conditions seen in the data (+0.5°C), although slightly more in the south at 18.6°N, warmer conditions are depicted in the data (+0.8°C) whereas the model barely finds any change (+0.12°C). In the North Atlantic, only two data points are available in the alkenone database, and the one North of Iceland falls on the model land point. The last point is located south of Iceland, at 58.76°N and depicts a warm anomaly of 0.2°C. The modelled SST anomaly is negative on the annual average (-0.61°C), but with warm anomalies occuring in July (+0.07°C), August (+0.8°C) and September (0.18°C), i.e. in summer, as expected for alkenones in northern high latitudes.”

Page 1372, line 22. “If you are to ascribe confidence from the closeness to the PlioMIP multi-model mean then the reader needs to be able to assess this. This could be achieved from a figure, table or some statistical measure.”

I suggest we remove this sentence, since our results are not directly comparable neither to PlioMIP experiment 1 (different SSTs), neither to PlioMIP experiment 2 (AOGCM

vs AGCM). The same comment can be made regarding the comparison with PMIP 6k results.

Page 1374, line 5. “The comparison with the results of Krinner et al., 2012 is further detailed in the discussion, but some of this should be moved to this section. Otherwise the reader is left wondering why there should be differences between these two studies and whether the results described in this section are robust.”

We agree that the structure here is a bit confusing. The sentence “This result is different from Krinner et al.(2012), in which an increase in precipitation due to MLC and wetlands is depicted on the zonal average, especially from 15° to 25°N.” is now closing this paragraph, accompanied by “The differences between the two studies and the possible reasons for divergent results are discussed in the Discussion section.”

Page 1377. “What do the differences between Krinner et al., 2012 and this study suggest for the Pliocene simulations presented here and the robustness of the paper’s conclusions?”

It is true that the robustness of this study should be better addressed, and we thank the reviewer for this useful comment. First, boundary conditions between the two studies are different (lake + wetlands in Krinner, lake only in Contoux, and the shape of the MLC is also different). Vegetation and SST boundary conditions are slightly different, although both mid Holocene. Although the atmospheric model is the same, the boundary layer parameterization in Krinner et al. 2012 was changed in order to simulate more convective precipitation in the region (and thus be in better agreement with precipitation data in the Sahel, see Krinner et al. 2012 figure1, compared to our version of the model, which is the state-of-the-art version, and slightly underestimates the annual amount of precipitation, figure 3c). As noted in the discussion, several other studies only find a local effect of the presence of a megalake (Sepulchre et al. 2009), and of megalake + wetlands (Coe and Bonan 97, Brostrom et al. 98). Nevertheless, a robust feature is what happens above and in the direct vicinity of the lake (i.e. reduction of deep convection because of colder surface, and redistribution of moisture at the north and east via increase surface winds and anticyclonic circulation), which is also visible in all the other studies, including in K12, although to a lesser extent. These mechanisms take place probably everytime that a megalake is present in a warm climate. Remote impact of the megalake Chad alone or with extended wetland area is mostly not simulated in these studies, but the response of climate to this forcing is dependent on boundary layer parameterization and probably also on convection parameterization (Chikira et al., 2006). The discussion section has been rewritten in order to include this description of the robustness of the conclusions (see response to the other reviewer). Comparing the two studies highlights the importance of boundary layer parameterization on the response of climate to surface boundary conditions, but also allows the identification of robust features.

Page 1395, Figure 8 caption. “Are the differences plotted in this figure due to differences in the lake free vegetation distributions or is the lake roughness predicted? An extra sentence or two in the caption would tell the reader what they are seeing in this figure.”

We agree with the reviewer that this is unclear. The differences between mid-Holocene and Pliocene are due to different lake free vegetation distributions. This sentence was added in the caption of figure 8.

Interactive comment on Clim. Past Discuss., 9, 1363, 2013.

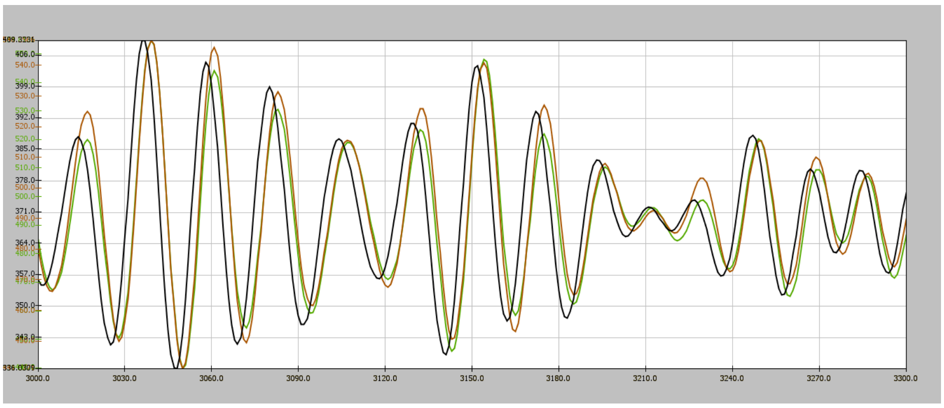

Fig. 1.