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Climate of the Past Discussions

Interactive comment on "Late Pliocene lakes and soils: a data – model comparison for the analysis of climate feedbacks in a warmer world" *by* M. J. Pound et al.

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This paper by Pound et al. describes new surface boundary conditions available for Pliocene climate simulations, as well as preliminary model results using these new boundary conditions. This topic is very welcome and is fully in the scope of Climate of the Past. This soils and lakes database will certainly be very useful for further modelling work. There is an important work of data compilation behind this, and it is much appreciated. In addition, turning the ArcGis database into netcdf boundary conditions and integrating them into the climate model must have been hard work, which should also be acknowledged. Overall, I have no doubt that the work behind this paper is of

good quality and represents a new step forward for the Pliocene community. However, the paper in itself does not reflect the amount of work done and the serious of this work, especially on the database. Both aspects (database construction, in particular for lakes, and analysis of model results) are treated very lightly, and the goal of the paper, which is to 'explore the feedbacks of soils and lakes on the climate of the late Pliocene' is not reached. Thus, a thorough re-writing of the manuscript is required before it can be accepted.

On the database side, details on lake extent calculations (dry and wet) and discussion on spatial and temporal uncertainty, especially for large lakes (megalakes) are needed. This database is going to be used as a reference, so the assumptions you make to calculate your extents needs to be documented. It is also important that you assess the uncertainties or the unknowns, both spatial and temporal, especially for all megalakes (Chad, Fazzan, Makgadikgadi, Eyre, and Northwestern USA lakes), not only the Zaire megalake. On the modelling side, I do not think the paper does an 'analysis of climate feedbacks' as suggested by the title. Such an analysis implies to detail the mechanisms underlying temperature, precipitation and biome changes. This is too rarely done in the text, and there are no figures to illustrate those potential mechanisms. Moreover, the changes seen in the temperature and precipitation are not quantified nor correctly described, and some features are simply omitted in the text (especially changes occurring above the oceans). Nevertheless, this paper is a unique contribution for the Pliocene climate community, and paleoclimates in general, for it addresses features that are too often neglected. It is to my knowledge the first study focusing on the global impact of lakes and soils on paleoclimate. I am aware that my comments imply substantial additional work from the authors, but I do think they are relevant, justified and necessary to give the paper the scope it deserves. It should be published, but only after the following points have been addressed:

General comments

For the database: 1/ You need to detail how the lake surfaces were calculated, and for

both scenarios (wet and dry).

I did not check the extent of all lakes, but of the two lakes I checked, I cannot reproduce the extents you find, and that's a problem:

I calculated the extent of lake MegaFazzan, based on the Upper Pliocene extension (Figure 11 from Drake et al., 2008): the lake covers approximately on grid cell of the figure, i.e. roughly 27000 km^2. Drake et al. 2008 mention 135000 km^2 in the text during more humid periods of the Pleistocene. Hence I don't understand how you find the 152856 km^2 mentioned in the Supplementary material. What are your assumptions in order to find this extent? Moreover I think there is a problem with the longitude of the center of Fazzan (27°E, line 59 of Suppl.)

For lake MegaChad, you give 446760 km^2 for wet scenario and 296378 km^2 for dry scenario, and again, there is absolutely no way to know what are your assumptions in order to find these extents. Moreover, you cite Schuster et al. 2009, who mention a surface of 'more than 350000 km^2' for a maximum level of 325 m asl during humid periods, and Otero et al. 2010, who mention frequent connections between the Chadian and Niger provinces (via the outflowing of MegaChad in the Benue river) during the Pliocene based on results from Otero et al., 2009, PPP. Some information on lake surface is given in Ghienne et al. 2002, who give a relation between lake level, volume and extent in table 1, computed from the TOPO6 dataset. They find an extent of 448000 km^2 for a level of 321 m asl. Nevertheless, Leblanc et al. 2006 using another topographic dataset (SRTM 30), suggest an area of 340400 km^2 for a maximum lake level of 325 m asl (see Leblanc et al. 2006, text and figure 6). So, there is still uncertainty, not on maximum level, but on the maximum extent. This should be included in the discussion of uncertainties, and you should explain how you find 446760 km^2 for the wet scenario. For the dry scenario, what did you assume for the level? Schuster, 2002 describe alternating dry to wet episodes during the Pliocene in the Northern subbasin. I don't think one can have more information than that, so how did you do? I would probably have taken something like 290 m asl (i.e. a 'big Chad lake', with the

Bahr el Ghazal valley being inundated, although there is no way to know if that's a good assumption), which gives an extent of $\sim$140000 km^2 with Leblanc's curve, and $\sim$180000 km^2 with Ghienne's curve.

In short: what are your assumptions on extents? What process lies between the surfaces published in the literature and the extents you give? How do you calculate wet and dry extents?

2/ You need to assess as much as possible the spatial and temporal uncertainty, especially for large lakes: MegaFazzan + gulf of Syrt, MegaChad, Zaïre megalake, Eyre-Frome megalake, Makgadikgadi megalake. In the paper the discussion assesses only the uncertainty on the presence or not of water surface, and briefly about Zaire megalake. The authors do not discuss the uncertainties due to i) the interpretation of the lake extent by the authors of this study from the literature available (problem will be partly solved when you describe your calculation procedure) ii) the uncertainty associated to the upscaling for the creation of maps via the method of percentage of grid cell and iii) the uncertainty on the timing of certain lakes, which might not be present during every warm period or might only be present for a very short period of time (century to millennial time scale). A discussion of this would be much appreciated, in order to assess the validity of the database which will now be used as a reference by the community. The temporal variability of the megalakes and the fact that they were not necessarily present during all humid periods is also important if this database is to be used in PlioMIP phase II, which will focus on a discrete time slice around 3.2 Ma. 3/ It is necessary to show the soils map used in the control experiment, for comparison, otherwise the reader does not know where the changes are located. It would also be useful (if not necessary) to provide a map of soil albedo changes and soil texture changes (see Specific comment Page 3184, lines 4-6) for the reader to understand where the changes in temp/precip come from.

For the modelling experiments: 1/ You need to describe, even briefly, how lake surface is treated in your model. Are there several layers? How is the temperature of the

lake calculated? Has this model (MOSES2.2/TRIFFID with lakes) been used for other studies (paleo or present) and have the results been confronted to data?

2/ You need to quantify the changes seen on the figures in the text. There is not a single number in the text, just 'small degree of cooling', 'a warming', 'a strong warming' 'a slight cooling' 'a small decrease' etc.. Since the figures are hardly readable either, it is a big problem. For biome changes, you generally give more detail on the location and extent. The biome changes would be easier to assess if you could find a better way to represent them (see my Specific comment about figure 5).

3/ You need to talk about all the large features seen on the figures, especially changes seen over the oceans. For example, in the precipitation response to changes in soil and soil+ lakes, there are some strong modifications of the ITCZ, with +/- dipoles suggesting a shift of the ITCZ, particularly in the Pacific Ocean. There is also warming around Greenland and seasonal cooling in Antarctica. These features are not discussed in the text, do you think they are robust, or do you think it is an artifact? I doubt that the lakes or even soils can have such an impact away from the changes, and this is not what you claim in the paper. Whatever the reasons for these changes, you cannot omit to talk about it. My feeling is that these changes (as well as those over the Amazon basin maybe) could be related to internal model variability. You have to check if a longer climatological mean (i.e. 50 years or even 100 years, since you have 350 years of integration this should not be a problem) would reduce the differences which are seen away from the zones of soils and lake changes.

4/ You need to relate the changes in precip/temp to the changes in boundary conditions AND explain the mechanisms behind it (otherwise you cannot state that you do an 'analysis of climate feedbacks'). For example, you say 'lakes in Australia have created a small reduction of desert in this region' (page 3190, lines 12-13). However on figs 4 and 5, there are no changes in temperature over Australia, and a decrease in precipitation in Pliocene Wet lakes experiment. What mechanism is at stake then? Is it a difference in cloud cover? Minimum temperature?

In summary, I think it will be easier for you and for the readers to understand the underlying mechanisms if you provide more figures, in particular albedo and roughness changes for soils, but also for lakes (a lake surface is generally darker and flatter, and this can change temperature and winds, and hence modify pressure and precipitation). Section 3.3 needs to be re-written, changes in temp/precip quantified and related to boundary condition changes and explained. Discussion could also be improved. Notably, you compare your results with Krinner et al., 2012, which focused on the Mid-Holocene, but not to Coe and Bonan 1997, Bröstrom et al. 1998, Sepulchre et al., 2009, which also investigated lake Chad feedbacks in the mid-Holocene, neither to Burrough et al. 2009, which investigated megalake Makgadikgadi feedbacks during the LGM, or to Contoux et al. 2013, which is the first study to focus on the lake feedbacks in the Pliocene, although only for megalake Chad (see Specific comments).

Specific comments:

Page 3178, line 1: and also Contoux et al., 2013, which focuses on the Megalake Chad in the mid-Pliocene. By the way, the reference to Sepulchre et al. 2008 is wrong, it is Sepulchre et al. 2009 who investigates the feedback of the lake on the mid Holocene climate. Sepulchre et al 2008 investigates the water balance of the Chad basin under mid Holocene conditions, which is different.

Page 3178, line 4: You could also note that Sepulchre et al., 2009, Krinner et al., 2012 and Contoux et al., 2013 find a decrease of precipitation above the Megalake Chad, which is different from your results. You could also discuss the reasons for this discrepancy (note that Contoux et al. 2013 found that response away from Megalake Chad was dependant on boundary layer parameterization, but that the drying response over Megachad was robust)

Page 3178, line 21: 'As lakes and soils have had significant regional impacts on mid Holocene precipitation (e.g. Krinner et al., 2012), it stands to reason that similar affects could be seen in the Late Pliocene.' First, Krinner et al., 2012 do not change the

soils, so this citation is only appropriate for lakes, you should try to find another citation for soils. Moreover, the 'have had significant regional impacts' is probably a little too affirmative in light of recent results (Contoux et al., 2013) and of previous results (Sepulchre et al., 2009, Brostrom et al., 1998) which depict only a minor effect of lakes. 'could have had' seems more appropriate, to my opinion.

Page 3180, lines 6 to 15: this methodology seems appropriate. However, what about the influence of the parent material, which can be different in the Pliocene? It would be nice to include a sentence or two on this point.

Page 3183, lines 5-10: 'the initial vegetation pattern for the control run was prescribed from PRISM3D' is that true just for the control run or also for the other simulations? Please clarify.

Page 3184, lines 4-6: 'it is only if the soil type changes to one of a different color'. It would thus be very useful to include a map of soil albedo changes between the control and the Soils experiment. Same comment for soil texture (lines 9-10).

Page 3184, lines 24-25: "The BIOME4 model was driven from the average annual climate data obtained from the last 30 yr of each HadCM3 experiment'. So you did not use the anomaly procedure to force BIOME4? This seems inconsistent with previous work from Salzmann et al (2008) and Pound et al. (2011). Moreover, you do not precise which CO2 level you used to force the BIOME4 model, and which resolution you use.

Page 3188, line 14 : biome instead of 'BIOME'

Page 3188, paragraph 3.3.1 : about temperatures: There is also a cooling in Northern Red Sea, as well as the extreme East of the Arabian Peninsula on the annual mean. You don't talk about the warm oceanic anomaly surrounding Greenland in annual and DJF means. This feature has to be explained, and it seems to be robust because it is reproduced in the Soils + Wet lakes experiment. Moreover, all these features have to be related to the corresponding changes in soils. If you don't relate the temp and pre-

cip changes to soils changes, there is no point in explaining these differences. There are also temperatures differences over Antarctica in JJA, which are not mentioned and not explained. About precipitation: 'a small increase', 'a reduction': please quantify. Actually, the biggest changes in precipitation are over South America, as you mention, but also over the ocean, especially tropical Pacific and Southern Indian ocean. Once again, these features are not mentioned and not explained. In particular, the tropical Pacific anomaly seems robust because it is reproduced in the Soils + Wet lakes experiment.

Page 3189, line 8: 'a modest increase': please quantify, and on the color scale, it's the darkest blue, so it doesn't seem so modest.

Page 3191, line 9: 'the global distribution .. is significantly different from present day' especially for soils, this cannot be assessed on figure 2. Please include Control run soils map.

Page 3191, lines 17-23: even for the mid-Holocene, the effect of including large lakes is not obvious and is highly dependent on the boundary layer parameterization (see Discussion in Contoux et al., 2013). Interestingly, in Contoux et al. 2013 the effect of Megalake Chad on climate is similar in the mid-Holocene and the Pliocene. You could use this result to strengthen the hypothesis you make that similar processes may operate in the mid-Holocene and Pliocene.

Page 3191, lines 23-24: it would be very surprising that you get the same results than Krinner et al. 2012 using a different model (coupled vs atmosphere-only) and different boundary conditions (Krinner et al., 2012 uses lake and wetland extent reconstruction) in a different setting (Pliocene vs early Holocene).

Figures

None of the figures except figure 5 have longitude and latitude marks. This is a problem, because it is difficult to point the location of the lakes/soils and the corresponding

simulated changes.

Fig 1: you should also precise that triangles are soil data and circles lake data.

Fig 2: the map of soils used for the control simulation is necessary, to assess the differences between Pliocene control and Pliocene + soils simulations.

Fig 3 and 4: the caption of these figures should be something like this “Top : mean annual . . . Middle : same for boreal winter (December to February). Bottom: same for boreal summer (June to August).” Moreover I think these figures are really too small (I have to zoom up to 600% on my screen, and the colors are quite blurry), and there are no marks on the maps. The scales are quite unusual too.

Fig 5: Once again I think the panels are too small. The key for the biomes cannot be read unless zooming at +500%. Suggestion: the changes are small between each panel. It would be easier to locate them if you could find a way to plot only the grid cells where the biome is different than the Pliocene control.

Supplementary material

If the future of this database is to be used in climate models, it would be useful to know if one can get the netcdf files for lake percentage and soil coverage, and from where one can get them.

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