Geosci. Model Dev. Discuss., 5, C265–C268, 2012
www.geosci-model-dev-discuss.net/5/C265/2012/

Geoscientific
Model Development
Discussions

Interactive comment on "Modelling mid-Pliocene climate with COSMOS" *by* C. Stepanek and G. Lohmann

Anonymous Referee #1

Received and published: 31 May 2012

This paper describes the standard Exp 1 and Exp 2 PlioMIP simulations, as carried out by the COSMOS model. The paper follows the PlioMIP Special Issue guidelines, and also includes a couple of interesting sensitivity studies.

(1) General Comments:

COSMOS – please put the model version number in the title.

In the abstract – 'warmer and wetter' is a bit general – say 'warmer and wetter in the global mean'

Pg 920, line 13. 'Our version' of COSMOS sounds a bit disconcerting. How does it vary from the 'standard' version. Does it have a version number or reference? You cite Roeckner et al for the ECHAM5 atmosphere, but here you are using it at a different

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

resolution to that described there? Please be clear throughout the model description which aspects are 'unique' to your version of ECHAM/COSMOS, and how they vary from the standard version. Ideally provide a reference which described your exact reference.

P922, line 5. What is the reasoning behind using a solar constant of 1365W/m2 in atmosphere-only, and 1367 in the coupled model? You address this later in the sensitivity studies, but I just wondered why they are different values.

P923. As for ECHAM, please do the same for MPI-OM. Is there a model description paper which shows e.g. climatologies for your particular setup, e.g. your particular resolution, position of poles etc. If not, please be very clear how your version differs from that in the referenced papers.

P924. Other studies are listed which used a 'comparable' version of the model. Do you mean 'identical?' If they did differ, how did they differ?

P925 line 9. Not sure what you mean by ECMWF (United Kingdom)? Do you have a proper reference?

P926. Line 9. When you say 'minor' influence on the climatology, do you mean minor influence on the delta Pliocene minus preindustrial? I would expect a difference of 2W/m2 in solar constant (i.e. $\sim$0.5 W/m2 radiative forcing) to have a significant effect on the absolute climate. This is discussed again on p939. You say the temperatures are identical to 2.d.p – this is actually quite surprising given a forcing of 0.5 W/m2 ?

P926, line 20. If you are preserving the PI land-sea mask, then this sounds more like the 'alternate' version of experiment 2. In fact from the figure it looks like you have a 'hybrid' alternate-preferred setup, in that the MAJOR land-sea mask changes (Hudson Bay, West Antarctic) changes are implemented, but more minor changes realted to sea-level change are not included. Maybe it could be phrased like that?

P927, line 20. sub-gridscale effects – did you apply a scaling such that the sub-

GMDD
5, C265–C268, 2012

gridscale Pliocene topography (derived from relatively course Pliocene map) had the same magnitude as the PI sub-gridscale parameters (derived from relatively high resolution modern observations)? It might be good top plot the Pliocene vs. Modern subgridscale fields implemented in the model.

P928. I don't understand 'Our method assumes that at any location a warming of the PI ocean surface will fully remove the sea ice cover'. This is discussed further on pg 940, but is still not clear. I am not convinced that just a small woarming should lead to seaice removal. What if the seaice is very thick?

P930. the method of converting biomes to JSBach is interesting. Did you perform a regression, or was the conversion of parameters done 'by eye'? in addition, it is not clear to me if your method is effectively based on anomalies, or absolutes,. e.g if you took your regressed parameters for converting biomes to JSBach fields, and then applied these same parameter to the modern, how close would your new modern JSBach vegetation fields be to the original fields? Identical? Or similar? Can you plot these? e.g for comparison with figure 4a,c? indeed, it would be interesting to compare a short atmosphere-only run with the re-calculated preindustrial values, and this would give a feeling for the uncertainty introduced by the mis-match in vegetation classifications. Again, this is discussed a little later in the manuscript. . ..

P931, line 10. co2 – most importantly, 405ppmv is the pliomip standard, and used by other groups in pliomip. This is probably really the reason you used that value!

P932. the TOA inbalances – in the atmosphere-only run, this is not really an 'energy gain' but an energy inbalance – the model does not gain energy at this rate, it effectively also throws the energy away because the SSTs are fixed. In the atmosphere-ocean case, this is actually an energy gain (assuming the energy budget of the model is closed). Not sure what you mean by 'inherrent feature of the model forcing'.

Typos

GMDD

5, C265–C268, 2012

Artifiacts -> artefacts? P927 line 10.

P932. frame -> timeframe or framework.

P932. residuum -> residual?

Interactive comment on Geosci. Model Dev. Discuss., 5, 917, 2012.

GMDD

5, C265–C268, 2012