Clim. Past Discuss., 7, C1077–C1083, 2011
www.clim-past-discuss.net/7/C1077/2011/

Climate of the Past Discussions

Interactive comment on "The Middle Miocene climate as modelled in an atmosphere-ocean-biosphere model" *by* M. Krapp and J. H. Jungclaus

N. Herold (Referee)

nicholas.herold@uclouvain.be

Received and published: 14 July 2011

Krapp and Jungclaus describe simulations of middle Miocene climate conducted using a coupled ocean-atmosphere model. Such models have until recently been rare in deep time paleoclimate research, particularly for the Miocene, and are a fundamental addition to the area. The experiment design is sound. The subject of the paper is certainly within the scope of Climate of the Past and I believe this can be a very useful contribution, however, I feel the text needs significant revision. It seems to skim over some of the important/interesting details in the model output. Particularly, differences are described between the simulations but with little breakdown of the model diagnos-

tics to find out (if possible) why, or without discussion of what the ramifications of such changes may be (for proxy interpretation or for maintaining lower temperature gradients for example). More in-depth analysis is required and the conciseness of the text needs improving.

The goal of this study is to see if a meridional temperature gradient closer to proxy records can be achieved using a dynamic ocean model (as opposed to previous studies) and to see how topography/bathymetry and CO2 contribute to Miocene warmth. However some results, such as winter storminess, while interesting are not linked by the text in any obvious way to poleward heat transport. Thus more in-depth analysis is needed or the associated figure could be excluded without subtracting significantly from the paper. The response from the ocean is really interesting and here a lot more could be said. Namely, the near-modern AMOC at 360 ppmv and its collapse at 720 ppmv, and the consequent reduction in poleward heat transport, need analysis. The authors include plots of the zonal and meridional volume transports, which are very useful, but do not explore why this collapse occurs, and perhaps more importantly why the AMOC is at near modern strength at 360 and 480 ppmv despite the Panama gateway. Of course, this has something to do with the increased salinity in the Atlantic as implied. Looking at the surface winds I imagine surface flow through Panama is westward. If it's believed that Tethys outflow is involved (e.g. Schnitker 1980) then this should be examined. Looking into this seems useful in the context of examining mechanisms responsible for lowering the equator-pole temperature gradient.

It can be difficult to decide which aspects of the output to focus on from such models. As the authors state, the addition of a dynamic ocean is what is novel here, so it would be nice to see what insights can be gained from this. While future sensitivity studies or analysis may be conducted regarding bathymetric changes, I think it is still appropriate to examine the results to the extent that the main climatic changes can be understood.

Specific Comments

- The abstract needs revising. The 4 – 8% net increase in poleward heat transport isn't actually even discussed in the body of the text. The conclusion that a CO2 higher than present is needed to qualitatively support Miocene proxy temperatures isn't new. Instead, it would be good, again, if insights could be gleaned from these results in order to add to our understanding of Miocene climate.

- The Tripati et al. (2009) reference is not for temperature, they reconstruct CO2, look in there for references to the 3 – 6 deg. warming. In any case, I don't think global mean temperature should be compared to this warming since this is based on bottom water temperatures and high southern latitude SSTs.

- While it is true that "The large forest cover and the northward extension of forest is in line with vegetation reconstructions (Wolfe, 1985)." This is stated several times as though it were surprising that a high CO2 would lead to an overall increase in forest cover. This is again stated in the conclusion; "Assuming higher atmospheric CO2 levels, we are also able to reproduce the densely wooded Middle Miocene." While this is true it is very qualitatively proven (and again, not surprising) and doesn't seem to warrant mentioning in the conclusions, or at least not without more rigorous examination.

- It would be useful to cite Butzin et al. (2011), as well as Micheels et al. (2011) who also use a coupled model. While the latter is for the late Miocene it could provide a useful comparison. I also believe the authors compare their results to the same or similar dataset as Micheels et al.(?) What age range do the data that are used for comparison span?

- Please add a sentence or two describing the vegetation model, particularly how many vegetation types it simulates. As a side note, could the soil properties in the biosphere model not have been globally averaged to provide a 'level playing field' for all grid points? This may or may not be more accurate but would help with places like the Sahara.

- Page 6, lines 9 – 19: Henrot et al. (2010) use initial conditions from an OGCM with

altered bathymetry, thus their predicted SSTs reflect these changes to some extent. I think it is worth mentioning this as it shows an evolution in complexity from studies like You et al. (2009) and Herold et al. (2010); with your study being the next step.

- A sentence on the metric used for determining model equilibrium is needed.

- Comparing absolute values between different models isn't so useful (e.g. page 9, line 24 and page 6, line 24), You et al. (2009) use a modified ocean heat flux (not based on a dynamic ocean). Better to compare model sensitivities if possible.

- Page 10: include an additional sentence or two (or rephrase the last paragraph) explaining more the steps involved in attributing the temperature change between each set of two experiments (in Fig. 11) to albedo, heat flux and emissivity. Just so people don't need to go to Heinemann et al. (2009) for the process.

- I think it would be fair to place much more emphasis on the cold bias that has almost certainly effected the low latitude marine SSTs in your model-data comparison. In fact, why not use the SSTs by Stewart et al. (2004) (which are much warmer) in your comparison or as an example of well-preserved samples?

- The proxy comparison needs work, model-data plots have been made but it is not stated which model performs best. Where are the majority of model-data discrepancies? Is there a reason for this?

- The conclusion reads like a summary (which is what the previous section was). Here points need to be made that draw together what has been discussed and perhaps indicate some details of what is to be done next. Also, there doesn't seem to be any statement as to how well or how poorly the model recreates the meridional temperature gradient suggested by proxies, as mentioned in the introduction. This should be stated and if temperature gradients are too high compared to proxies then perhaps some discussion of why this is so should be included (in the discussion).

- Where conclusions are consistent with previous studies, this needs to be stated. Even

if certain results have not been shown for the Miocene, they have been shown for other time periods (e.g. Eocene, Pliocene).

Technical Corrections

- Page 2, line 22: “wet to very wet” isn’t informative, please rephrase.

- Page 3, line 10: Herold et al. (2010) did reproduce a low temperature gradient because prescribed SSTs were used.

- Page 3, line 27: Should be “Section 5”.

- Page 5, line 17: Remove commas from sentence.

- Page 6, line 14: Should be “SH_700”?

- Page 9, line 6: is the “19.2” meant to be “19.8”?

- Figure 5 and 6 are cited before figure 4.

- Caption for Fig. 5: Should part C be “MIOC480 compared to MIOC720”? Also the continental distribution needs to be changed for these figures.

- The title “Role of topography” is misleading since you are also discussing bathymetry. Perhaps use “topography/bathymetry”?

- Line 18 Page 8: rephrase to “is not an enclosed basin”

- I don’t believe sentences like Page 11, lines 8 – 9 and page 7, line 2 are necessary.

- Page 11, line 18: Rephrase to “... an enhanced hydrological cycle and stronger greenhouse effect can be attributed to higher CO2”.

- Figure 13: I assume the lines at each point are error bars, perhaps try to make these lighter versions of the green, red and blue already used so that the actual data points aren’t covered.

- Page 14 last line: should be “may differ”.

- Figure 9: The dotted boxes showing the Panama and Tethys gateways also need to be in 9b.

- Side note: Might have been interesting seeing 720-360 ppmv plots instead of 720-480 since the former is a doubling and the common yardstick.

References

Butzin, M., G. Lohmann, and T. Bickert, 2011: Miocene ocean circulation inferred from marine carbon cycle modeling combined with benthic isotope records. Paleoceanography, 26, PA1203.

Heinemann, M., J. H. Jungclaus, and J. Marotzke, 2009: Warm Paleocene/Eocene climate as simulated in ECHAM5/MPI-OM. Clim. Past, 5, 785-802.

Henrot, A. J., L. François, E. Favre, M. Butzin, M. Ouberdous, and G. Munhoven, 2010: Effects of CO2, continental distribution, topography and vegetation changes on the climate at the Middle Miocene: a model study. Clim. Past, 6, 675-694.

Herold, N., R. D. Muller, and M. Seton, 2010: Comparing early to middle Miocene terrestrial climate simulations with geological data. Geosphere, 6, 952-961.

Micheels, A., A. A. Bruch, J. Eronen, M. Fortelius, M. Harzhauser, T. Utescher, and V. Mosbrugger, 2011: Analysis of heat transport mechanisms from a Late Miocene model experiment with a fully-coupled atmosphere-ocean general circulation model. Palaeogeography, Palaeoclimatology, Palaeoecology, 304, 337-350.

Schnitker, D., 1980: North Atlantic oceanography as possible cause of Antarctic glaciation and eutrophication. Nature, 284, 615-616.

Stewart, D. R. M., P. N. Pearson, P. W. Ditchfield, and J. M. Singano, 2004: Miocene tropical Indian Ocean temperatures: evidence from three exceptionally preserved foraminiferal assemblages from Tanzania. Journal of African Earth Sciences, 40, 173-189.

Tripati, A. K., C. D. Roberts, and R. A. Eagle, 2009: Coupling of CO2 and Ice Sheet Stability Over Major Climate Transitions of the Last 20 Million Years. Science, 326, 1394-1397.

You, Y., M. Huber, R. D. Müller, C. J. Poulsen, and J. Ribbe, 2009: Simulation of the Middle Miocene Climate Optimum. Geophys. Res. Lett., 36, L04702.

Interactive comment on Clim. Past Discuss., 7, 1935, 2011.