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Interactive Comment

Interactive comment on "The challenge of simulating warmth of the mid-Miocene Climate Optimum in CESM1" *by* A. Goldner et al.

Anonymous Referee #2

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The challenge of simulating warmth of the mid-Miocene Climatic Optimum in CESM1.0

This paper presents a set of experiments using CESM1.0 designed to explore the ability of the new model to produce a past climate state. Whilst the approach is interesting and important I have sufficient concerns about what has been done to suggest that major revisions are required before the paper can be published.

Concerns:

1. I am confused by the use of fluxes from a CCSM3 experiment in the new version of the model. If the CCSM3 experiment was unable in itself to reduce the SST gradient sufficiently, using the fluxes from that experiment in the slab ocean version of the new CESM1.0 seems to bias the new model towards not attaining the kind of gradient re-

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ductions required to match the observations. It is any surprise then that the new model cannot do what the observations, on the face of it, seem to require?

2. Overall I feel there needs to be a far better appreciation of the bias introduced into the comparison of observations and model output by being so inclusive with the observations. I do not see why the climate simulation should match observations from 3 million years before the MMCO when the model has been set up with, presumably, a best guess for the MMCO itself. It seems to me that a lot could change over those kinds of timescales which would be highly relevant in terms of the forcings given to the model.

3. The attribution of the same tropical state in the Miocene as in the Pliocene is certainly conjecture in the absence of sufficient observations to support the premise. The reference to Pliocene tropical SSTs being 4 to 6 C warmer than present day does not match my own understanding of those papers which present data specific to upwelling zones in the tropics but that is not all of the tropics.

4. In section 3.1 reference is made to a PI simulation - what PI simulation is this?

5. I am not sure that 51 sites with a poor global coverage encompassing the last 6 million years would actually give a valid estimate of modern global mean temperatures. Therefore, the use of this technique to support the method of deriving observational-based estimates of Miocene global temperature seems very unproven to me.

6. I see nothing convincing that the error bars given on the observations will encompass all the temporal variability over such a large timeframe. This seems more like a statement of hope rather than proven statement. My view is that uncertainties in the observations are too lightly treated. Some of the fundamental questions are ignored, for example are the observations really of MAT or a growing season? This is especially important for high latitude sites where it seems to me far more likely that you will have a seasonal bias in the observations, rather than any representation of MAT. If so the provided observational error could fall short of reality at such locations.

8. The method of DMC seems strange. By plotting absolute temperatures the effect of latitude on temperature will blur the assessment of model performance (make it look better than it actually is). A better test might be compare anomalies in the observations versus the anomalies from the model.

9. The combination of forcings in one of the sensitivity tests is interesting but I am left wondering how realistic that combination of forcings would actually be in the context of the MMCO. It seems like a 'throw everything at it including the kitchen sink'-type approach, but the validity of such an approach is not discussed.

10. Given the inherent uncertainties in the observations, which are not fully accounted for (truthfully because it is probably not possible to do that at the current time – if so the conclusions need to be more temperate to take account for that), the incomplete nature of the sensitivity tests, the use of a slab ocean model, and the fact that the entire analysis has been conducted with just a single model (thus model dependency is not explored), leaves me unconvinced about the basic conclusion that the model is too insensitive. The point about missing forcings I can agree with, as this seems sensible given the wide timeframe considered, but there seems little within the paper to support the premise that the sensitivity of CESM1 is somehow fundamentally flawed. Also, I think the paper would benefit by having real input from the scientists who created the observational datasets that the model is compared to. That way a more convincing case can be constructed that the error bars are reasonable.

Interactive comment on Clim. Past Discuss., 9, 3489, 2013.

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