Clim. Past Discuss., 8, C2010–C2013, 2012
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Climate of the Past Discussions

Interactive comment on "Large-scale features of Pliocene climate: results from the Pliocene Model Intercomparison Project" *by* A. M. Haywood et al.

C. Brierley (Editor)

c.brierley@ucl.ac.uk

Received and published: 23 October 2012

Summary

I would like to invite the submission of a revised paper for publication in Climate of the Past. The comments of the three referees are distinctly positive in recommending this work as fit for publication after revisions. One issue raised and that I found too, was the readability of some of the figures due to issues of scale. This is mainly an editorial issue, so I don't see that the authors need worry too much. However, may I recommend that you label the separate panels (a, b, . . .) and refer to them as such rather than by their position. This will let us span them across multiple pages if needed.

Actions following Author's Responses:

1). Ran Zhang's comment will be addressed in part by subsequent publications and in part by the inclusion of a revised simulation from the GISS model (at least I hope that resolves it). Please do state that further data-model will be the focus of future work at the end of section 4. Data-model comparison is an important task with respect to these simulations. However, I feel that this article does not need to provide the full analysis and that would/will be a paper in of itself. By including the correlation coefficients and citing the source of the palaeoclimatic data, I feel the Authors will have addressed James Annan's comments.

2). Referee Levy had two specific comments (in the supplement) that I feel you need to address. The first, about the specification of the WAIS, probably requires a single sentence or citation to address. Secondly, he wonders if the Antarctic MAT (that you consider as potentially spurious) should be removed from the analysis. I do not mind whether you remove it or not, as long as you provide a justification for your choice (to me).

3). Referee Williams made a couple of comments about the phrase "mid-Pliocene Warm Period". I have commented on the use of the term for the first reason elsewhere myself and personally favour mid-Piacenzian. I feel you've misunderstood his second point in your Author Response. When capitalised, Period has a precise stratigraphic meaning (and it is higher up the foodchain than Epoch), however, when uncapitalised you can use it in its more general context. I would hope that you are able to address his subsequent comments in a revised conclusions section.

4). Referee Huber makes several substantive points and both he and the authors have obviously discussed some of the issues. I would expect the following issues dealt with in a revised document:

4a). There is no information to let the reader judge whether each simulation has reached equilibrium (a fact that plays an important role in the climate sensitivity discussion). The proposed inclusion of global energy balances should address this and it needs to be discussed in Section 5. The related point about the evaporation in the fixed SST experiment should also be emphasized.

4b). I can understand Prof. Huber's concern about the treatment of model errors, although perhaps I don't feel as aggrieved as him. I was surprised that you used 2 sigma so freely (if fact you don't even define what you mean by it). I can understand your reticence against using more detailed statistical measures, especially since this paper is only intended as an introduction to the ensemble. I would expect subsequent data-model comparisons to go into much more detail. However, I wonder if you may not be better presenting the data in a format with less implicit assumptions:

i - For example, it may be better to present the bars in fig 5 & 6 as the range seen across the ensemble. Whilst not ideal, I think the limitations of the analysis are much more intuitive and it would have the additional benefit of making any non-normality visible. As the authors mention, the supplement shows each anomaly pattern.

ii - I wonder whether it is best to show 2 standard deviations as your measure of model spread. Other equally valid options would be a single standard deviation or the variance. This measures show the same information, but do not lead one to presume normality and thence to convert them into confidence intervals. I do not know what approach the upcoming IPCC report is taking for its atlas, but perhaps that would provide some guidance.

4c). Discussion of the significance of concordances and discordances in the data-model comparison should also be modified. Following the comments from Prof. Huber and the Authors' response, there seems agreement that this should be done.

4d). I do not feel it necessary (or practical) to repeat the experiments with varying levels of CO_2. However, I do think that it is worth mentioning that the level of 405ppm now appears in the upper range of more recent estimates – if only to note that the discordances would be even higher over high-latitudes with lower CO_2 levels.

Additional Comments

I found it surprising that you already reference Pagani et al. (2010) for the CO2 levels and Hansen et al. (2008) for "Charney" sensitivity, but make no mention of either when discussing and defining the Earth System Sensitivity.

Interactive comment on Clim. Past Discuss., 8, 2969, 2012.

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Interactive Comment