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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.

A. M. Haywood et al.

earamh@leeds.ac.uk

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On behalf of the co-authors I would like to thank the reviewer for his thoughtful consideration of the paper and his comments. Also I would like to thank him for the further discussions we have had on this subject leading up to the submission of this reply. These discussions were very fruitful and are always enjoyable.

In the interests of brevity I will condense the reviewer's comments to the most salient points:

1) The reviewer would like us to add information on the TOA energy budget (plus residuals) for the ensembles. We would be happy to do that in a table. This also links into the question of models reaching equilibrium. The reviewer is aware of the challenges in

this regard in a co-ordinated model intercomparison project (and in general). PlioMIP specified minimum integration times for all experiments which each group conformed to, which as a control was the best that could be hoped for. But adding the TOA will be an acknowledgement of the inevitable fact that some models will be closer to equilibrium than others. The effect that this may have on calculated Pliocene sensitivity can then be considered. The inclusion of TOA and residuals will also resolve a number of the other queries of the reviewer.

2) Regarding the choice of CO2 at 405. We would not state that the choice biased the results. There is an approximately 100ppm uncertainty range in the proxy data for CO2 that we have at the moment. In 2008, when the experimental design was established, most of the newer CO2 proxy records were not published. Traditionally, CO2 values in Pliocene OAGCM studies had been set around 400 ppm. However, the 405ppm value is not inconsistent with these newer records or records being published soon that show CO2 variability over orbital timescales. Of course PlioMIP recognises that CO2 is an uncertainty and will be a high priority for further sensitivity tests in the second phase of the project. However, it is not something that we can reasonably be expected to consider at this stage since it has taken 4 years to get to where we are (2 simulations with 1 CO2 value from 9 different groups). MIPs are not fast!

3) Data/model comparison. As stated in the response to a previous reviewers comment data/model comparison is not the main focus of this paper, since it is being dealt with more fully in a paper to be submitted in the near future. However, we will happily provide further clarification of the methods used to produce Figure 6. The fact that there is no ideal way to perform these kinds of comparisons (as noted by the reviewer himself) reflects why we decided to show the comparison in a few different ways. In Figure 5 we show the point-based comparison of MMM anomalies to proxy-based anomalies and also the point based comparison of data versus model anomalies as a function of latitude including the ensemble variation (for SST and SAT). In Figure 6 we aggregate the data and models into regions (in recognition of the fact that point-based comparisons

have their own limitations). We also acknowledge the fact that the MMM comparison to data in specific regions could be biased by the results of a particular model by showing each models SAT and SST anomalies individually in the supplementary information. Therefore, the reader is able to see for themselves how the DMC could be influenced by specific models within the ensemble.

Whilst there are a number of methodologies and statistical approaches/options available, our assessment is that the basic trends shown in the DMC will not change. If anything the reviewer appears to more curious as to whether or not the emphasis and expression we have currently placed on the outcomes of the DMC so far is treated in the best way. We think that this is a reasonable position and will adjust the text in the abstract, DMC section and conclusions to point out the fact that there are clear areas of concordance and discordance, but the variation within the model ensemble is sometimes quite considerable.

We do not see great value in exploring different statistical approach to the DMC if we provide clarification and a subtle reemphasis of what is already in the paper, this is especially true given the issue that we identify clearly in the paper regarding the comparison of time averaged proxy temperature estimates to model predictions run with a discreet set of co-varying boundary conditions. No matter what approach is taken (or its complexity) apples will always be apples and oranges will always be oranges. Until we have new data sets of time specific proxy data we will simply be comparing apples and oranges. The reviewer is quite correct though in pointing out the fact that this does not negate the option to reemphasise the outcomes of the DMC that we have performed so far, and then go on to say (as we do say) that these outcomes must be treated with caution because of the discontinuity between data and models.

As the reviewer will have noted, we have also not been able to include quantitative estimates of error on the proxy temperatures since they do not exist (only qualitative estimates of confidence). Therefore, our DMC is already assuming that the data is always perfect (due to a lack of information to the contrary). Given that there will

be some degree of error in the proxy estimates, its enforced omission makes it likely that the signal of discord between data and models already shown in our paper is an overestimate of the true discord if error estimates in the proxy records were available for consideration.

We hope that these steps will reconcile the valuable comments and suggestions of the reviewer.

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