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Climate of the Past
Discussions

Interactive comment on "A return to large-scale features of Pliocene climate: the Pliocene Model Intercomparison Project Phase 2" *by* Alan M. Haywood et al.

Anonymous Referee #2

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The manuscript by Haywood et al. presents results from the Pliocene Model Intercomparison Project Phase 2 and follows on an earlier Pliocene model intercomparison published in 2013. Despite the title, the manuscript focuses on two issues: Pliocene large-scale climate features and earth system/climate sensitivity. The study has the potential to be an important contribution. As presented, the study has several shortcomings, described below. My recommendation is to revise the manuscript, eliminating the analysis of earth system sensitivity and expanding the analysis of large-scale features and the comparison with proxy data.

General comments:

As an analysis of a model intercomparison project, the manuscript is fine. It reports the ensemble average and range across a spectrum of large-scale features including global, zonal and seasonal temperature; polar amplification; SST gradients, and precipitation rate, and compares them to pre-industrial conditions. These results are interesting and relevant but the analysis is rather cursory. It would have also been interesting for the authors to include some direct comparisons with PlioMIP1 in the Results (for example, include PlioMIP1 ensemble means in the figures and description of results). In the Discussion, there is some speculation about why the PlioMIP2 results differ from PlioMIP1, but it's just speculation. In addition, the analysis of PlioMIP2 models lacks investigation of why large-scale climate features differ among PlioMIP2 models. Both of these are major missed opportunities.

The estimate of earth system sensitivity (ESS) (equation 1) is not explained or justified. There is no a priori reason to think that the ESS will scale as the ratio of ln (560/280)/ln(400/280). This scaling would be appropriate if CO2 were the only factor changing between simulations. That this scaling is inappropriate is illustrated by the differences in ESS for CCSM4 (Table 2, compare CCSM4-2deg and CCSM4-UofT). In these two simulations with the same model, the Eo400-E280 SAT differs by 0.9 C due to difference in treatment of Pliocene boundary conditions. (No surprise.) The ESS estimate (equation 1) grows the difference to 1.8 C. Why would the same model respond so differently to an increase in CO2 of 160 ppm? If the authors believe this result is justified, they must demonstrate it by running these two CCSM4 Pliocene simulations with 560 ppm CO2. Given the shortcoming in the ESS estimate, all discussion of earth system sensitivity should be removed from the manuscript.

Specific comments:

Introduction. The Introduction could be improved. It does a poor job of justifying the rationale for conducting PlioMIP2. A strong case could be made that the PlioMIP2 offers an opportunity to evaluate climate models that have been strongly tuned for the present day, and to showcase advances in modeling since PlioMIP1. Instead, the Introduction (paragraph 2 specifically) is a clumsily written laundry list of all the publications that resulted from PlioMIP1. What's the point?

L. 156. The minimum integration length was specified as 500 simulated years. For many models, this is not sufficient time to reach equilibrium. I appreciate that the authors report the spin up time and net TOA radiation in Supplementary Figures and suggest that they add this important information to the manuscript.

L. 164. Here and elsewhere (e.g. L. 473) the manuscript mentions the release date of the model, and even makes statements like "the model sensitivity is more strongly related to parameterization choices and initial conditions than the release date of the model". As the authors are well aware, model performance is related to the accurate representation of the dynamics and physics and has nothing to do with the date of release. Please remove these confusing and unnecessary comments.

L. 237. "Lack of consistency in the seasonal signal of warming..." An analysis of the global average seasonality is not very useful (or at least ambiguous) here since the seasons are out-of-phase between hemispheres. Please show hemispheric averages, or just the NH average. Also, this is a place where additional analysis would be appreciated to understand the reason for the model differences.

L. 339. "...suggests that there are some inconsistencies between the way in which ECS and ESS were obtained." See my comments above about the estimate of ESS used in the manuscript.

L. 344. "each of these models provides a different but equally valid realization of ESS..." I don't understand this statement. Please elaborate or delete.

Data/Model Comparison. This section (lines 389-400) focuses on comparing the ensemble mean to the proxy data. How consistent are the models? It would be valuable to add a figure showing model agreement, the number of models that were within the criteria for a good fit. In addition, it would be valuable to estimate and report the good-

ness of fit for each model, as well as the ensemble mean.

L. 410. Please calculate and report the global mean DSAT/ DSAT estimate from proxy data for comparison. There are a number of ways to do this that have been reported in the literature.

Section 5.1. This section is quite interesting. In line 460-464, it is stated that there are differences between the Pliocene and RCP predictions. Please elaborate on these differences, in the same way that the similarities have been described.

L. 489. "Previous DMCs for the Pliocene..." I don't understand this sentence. Please clarify.

L. 545. "...suggest that SST data from the Pliocene tropics has the potential to constrain model estimates of ECS..." The discussion that follows (L. 550-577) about equilibrium climate sensitivity is not robust. ECS is calculated from a handful of local points from the same regions and is justified because it agrees with the ECS reported in IPCC, the exact value that this analysis should be testing.

L. 629. This is not a conclusion of this study, and therefore shouldn't be included in the Conclusions.

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