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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.

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We would like to thank Tim Herbert for providing helpful comments on this manuscript, which will help improve a revised version. We provide a response below to those comments that have been incorporated into version 1 of the manuscript:

[TDH1]: This is absolute scaling, but it would be more useful to provide a context: either add the absolute mean SAT, SST, or the scaling between delta SST and delta SAT(land). [line 41]

In the revised version we will specify by how much SAT increases over land and how much it increases over the ocean.

[TDH2]: Clarify? Meaning of “constraints” not evident. [line 44]

In the previous version there was a statistically significant relationship between a model’s Pliocene temperature response and the Equilibrium Climate Sensitivity when we excluded the EC-Earth3.1 model and averaged together the CCSM4 models. These were the modelling constraints we referred to. However, since the previous version of the manuscript the EC-Earth3.1 model has been withdrawn from PlioMIP2 and two further models have been added. This means that we now obtain a statistically significant relationship between a model’s Pliocene temperature response and the Equilibrium Climate Sensitivity without any ‘modelling constraints’. This sentence is therefore no longer needed in the revised version.

[TDH3]: Data shows polar amplification well equatorward of +-60 so this suggests model not able to capture all the physics? [line 43]

We reported the polar amplification as the ratio of warming poleward of 60° to the global mean warming. This is a standard metric for calculating polar amplification (Smith et al 2019). We do not state that there is no polar amplification equatorward of 60°, and figure 1b also shows that polar amplification occurs equatorward of 60°. Therefore, our paper does not imply model-data disagreement or any problems with model physics in this respect.

[TDH4]: This is all in the context of a constant 400 ppm forcing? E.g. no uncertainty in pCO2? Relevant to deducing ECS from SST- requires pCO2 estimate, correct? [line 48]

This is a good point and in a revised version we will add some further discussion about CO2 uncertainty to the text. The ECS estimate uses two inputs: 1. The data, which we assume comes from a 400ppm CO2 world. However, we note that uncertainties may mean the data represents a world where CO2 was slightly different. 2. The models which were all run with CO_2 of 400ppmv. Given that we do not currently have a range of model simulations with different CO_2 values, the only possible way of estimating ECS requires using regressions derived from a CO_2 = 400ppmv modelling world. We therefore currently have no other option other than to assume that the data also represents CO_2 of 400ppmv. If the data represented a world with a different CO_2 value it could not be related to the model outputs. However, the PlioMIP2 model design did accept that there are uncertainties on the KM5c CO_2 value (Haywood et al; this issue) and suggested that modelling groups also carried out experiments with CO_2 set to 350ppmv and 450ppmv in order to quantify CO_2 uncertainties. As the simulations with different CO_2 values become available, we will be able to add reliable error bars to our ECS estimates that will account for CO_2 uncertainty.

[TDH5]: Of course, given present proxy CO_2 data, we don't really know if this window was say +- 20 ppm higher than the canonical 400 ppm. . . [line 115]

Please see response to TDH4.

[TDH6]: I would have thought that reduction in winter sea ice and/or lower land surface albedo would have generated a larger winter warming relative to the mean anomaly. [lines 230-240].

The amount of winter sea ice has very little effect on temperature. This is because there is no sunlight over the winter pole, and so the value of the surface albedo is irrelevant. The reduction in winter snow cover away from the pole will affect only be a small proportion of the northern hemisphere surface. Therefore, it can only have a limited effect on hemisphere averaged temperatures.

[TDH7]: I am a bit mystified here on the Foley/Dowsett data source precision. For example, see Figure 9 of Caballero-Gill et al.: at 3.205 Ma, SST at Site 1125 is ∼21oC and 594 is 14oC. The Foley/Dowsett table gives 19.5 and 12.2 respectively. [lines 382-387]

Figure 9 of Caballero-Gill et al, includes data from 2.6Ma – 4.2Ma and as such data near KM5c only represents a very small proportion of the figure. The figure is therefore not of sufficient temporal resolution to obtain SST directly. The dataset upon which the figure is based for site 1125 (https://doi.pangaea.de/10.1594/PANGAEA.898162) shows that SST does nearly reach 21°C at 3.213Ma, however at 3.205 the SST is 19.5 as reported by Foley and Dowsett. A similar argument could be applied to site 594.

[TDH8]: Suggest moving to McClymont et al. data set in lieu of comments above? [line 397]

Sites which are included in both Foley and Dowsett and McClymont et al. datasets generally show the same or very similar SST estimates. There are some very small differences (0.1-0.2°C) which are undoubtedly due to the different time windows used (10ky and 30ky in Foley and Dowsett, 20ky in McClymont et al.). In some cases, McClymont et al. used previously unpublished data that could not have been included in Foley and Dowsett. As modellers, we need to validate our models against a wide range of different datasets. Here we choose to validate against the Foley and Dowsett dataset because the model results are already compared with the McClymont dataset elsewhere (McClymont et al., this issue). However, we note that the first order outcomes of the PlioMIP2 model-data comparison is the same, regardless of whether model results are compared with the Foley and Dowsett dataset of the McClymont et al. dataset. This will be noted in a revised version of our manuscript.

[TDH9]: To me, the pattern of data anomalies exceeding model anomalies near the gyre boundaries is quite robust (see MyClymont or example) and is a general feature of “warm climate” reconstructions- see Brierley for an earlier Pliocene time slice, and many others for the Miocene and Eocene. I think there is a fundamental model deficiency here. [line 401-406]

We agree that there could be a fundamental model deficiency in these regions and have stated near line 520 “The simulation of upwelling systems is particularly chal-

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lenging for global numerical climate models due to the spatial scale of the physical processes involved, and the capability of models to represent changes in the structure of the water column (thermocline depth) as well as cloud/surface temperature feedbacks". However, the interpretation of data in upwelling regions is also not trivial and we also discuss this in our paper.

[TDH10] and [TDH11]: See comments above – I think this SST data set is particularly problematic. [Lines 480-496]

Please see response to TDH7 and TDH8.

[TDH12]: F&D data set is more likely to be the issue. . .[lines 498-503].

Here the reviewer is referring to our analysis which shows that the mPWP-PI temperature anomaly can depend on which dataset we use to represent the preindustrial data. We note that if two different datasets give different values for the PI climate, than these datasets will lead to different values of the mPWP-PI temperature anomaly. This difference will be independent of which dataset was chosen to represent the mPWP. Because this paragraph was discussing the choice of preindustrial dataset, we do not agree that the mPWP F&D dataset will be an issue here.

[TDH13]: However this is not born out with modern sediments in the region so I think this is special pleading! Large compilations, the most recent being Tierney and Tingley (2018) do not identify an upwelling bias in the modern data set. [line 540]

The sentence referred to in the comment is: "This lends some credence to the idea that the observed mismatch between PlioMIP2 ΔSST and the F&D19_30 proxy-based anomaly could arise from the complexities/uncertainties associated with interpreting alkenone-based SSTs in the region as simply an indication of mean annual SST (Leduc et al. 2014)." The sentence was written as a possible explanation for the data-model mismatch. We also give other possible reasons for the data-model mismatch (i.e. that the model's struggle in upwelling regions). Although there is not a seasonal upwelling bias in the modern dataset, it is not implausible that such a bias could exist in the Pliocene. In a revised version of the manuscript we will continue to include this as a possible reason for the Pliocene model-data mismatch but will also include that no such upwelling bias exists in the modern dataset in order to include additional information for the reader.

[TDH14]: My concern here would be how much the use of the F&D data set alters this new estimate in comparison to Hargreaves and Annan? [lines 555-565]

This point refers to how the estimate of ECS depends on the dataset chosen. We agree that the estimate of ECS depends on the dataset chosen, but note that it also depends on the relationship between the published ECS and the Pliocene warming in the models. We suggest that using the new F&D dataset, instead of the PRISM3 SST anomaly field used by Hargreaves and Annan would improve the estimate of ECS. This is because the PRISM3 SST anomaly field was based on warm peak averaging over a 240,000-year timeslab while the F&D dataset better represents the MIS KM5c timeslice that our models are set up to represent. However, we will note in the revised version of the manuscript that as more orbitally tuned SST data becomes available it will be important to revisit the ECS analysis to ensure maximum accuracy.

[TDH15]: The quality of the estimation also depends on the reliability of the paleo-CO2 estimates

We agree that the relationship between ECS and ESS will only be robust if we are using the correct palaeo CO2 value. Please see the response to TDH4, which explains why we are not able to quantify errors due to CO2 uncertainties at the moment, but how we plan to do this in the future.

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