Climate of the Past Discussions

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Reply on RC2

Julia C. Tindall et al.

Author comment on "The warm winter paradox in the Pliocene northern high latitudes" by Julia C. Tindall et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2021-186-AC4, 2022

We would like to thank the reviewer for reading the paper and for providing useful comments. The reviewers comments are addressed below:

- *Despite the title and the focus on "high latitudes", the manuscript does not deal with all high latitude regions. There is a note to the Yallalie data (Perth, Australia) in Figures 1 and 2, but this is located at 30*S and debatable as "high latitude". Otherwise, there is no assessment of southern hemisphere high latitudes. Feng et al. (2017) cited in line 48 was likewise a study of high northern latitudes. The manuscript title and abstract need to clarify this focus.*

The reviewer is correct that we did not look at the Pliocene southern high latitudes. We will change the title to "The Warm Winter Paradox in the Pliocene Northern High Latitudes", and ensure that we mention 'Northern High latitudes' in the first paragraph of the abstract. We suggest that we continue to write 'high latitudes' in some other places, where several qualifiers are already used (e.g. line15, which states "Pliocene, high-latitude, terrestrial, summer temperatures"). However, we will change the Feng reference to refer to the Northern High Latitudes

- *The abstract is written very clearly, but would benefit from clarifying that the focus of the manuscript is not the whole Pliocene, but a narrower section of this epoch (e.g. lines 28-32 show that the focus is the Mid Pliocene Warm Period, and a shorter interval within it, "KM5c").*

We will rewrite paragraph 2 of the abstract as:

"We focus on the mid-Pliocene Warm Period (mPWP) and show that understanding the high latitude terrestrial temperatures is particularly difficult for the coldest months. Here the temperatures obtained from models and different proxies can vary by more than 20◦C. We refer to this mismatch as the 'warm winter paradox'"

- *line 15 might be easier to read with the removal of some commas: "For the Pliocene high latitude terrestrial summer temperatures, models and different proxies are in good agreement".*

Thank you for this suggestion. We will remove the commas as advised.

- *Introduction, first paragraph: the statements here are logical, but there is no supporting evidence given through citation of literature. Do any of these concerns appear in e.g. the most recent IPCC report, or perhaps discussed in papers looking at data-model (dis)agreement?*

In the revised version we will support these statements through citing:

McClymont et al 2020

Haywood et al 2016 . Integrating geological archives and climate models for the mid-Pliocene warm period. Nat Commun. https://doi.org/10.1038/ncomms10646

- *Line 29-30 indicates the focus in on the KM5c interval, but lines 75-76 indicate that proxy data for this narrow window is not feasible. This suggests that the model output is KM5c but the data is MPWP: this needs to be clarified here and in the abstract.*

We agree that this is important information, however we think it is too much detail for the abstract. The information will be incorporated here as suggested and we will change the paragraph near line 29-30 to:

"This paper will present a DMC for the Pliocene, focussing on the mid-Piacenzian warm period (mPWP, previously referred to as the mid-Pliocene warm period) which occurred between ca 3.3 - 3.0Ma (Dowsett et al., 2016). Most model simulations represent the KM5C timeslice (☐ 3.205Ma), although data will be less temporally constrained. The mPWP is the most recent example of a world which had CO2 levels similar to present, and was found by Burke et al. (2018) to be the most similar geological benchmark to global surface temperature predictions of 2030 CE. It is, therefore, a crucial period for model data consensus."

- *Line 35: this could refer to subsequent PlioMIP iterations as well as the first one.*

This is correct. We will reference PlioMIP2 here as well.

- *Paragraph line 40-46: this gives a good overview of the likely uncertainties which were assessed but not a sense of what impact these different uncertainties had (or not). Did any one / combination of the uncertainties give "most likely" cause(s) for some of the mismatches, or was it perhaps uncertainty or location specific? A line of two which notes the main findings of some of these publications would be helpful.*

This is a good question. The uncertainties were different at different locations; hence it is difficult to summarize concisely. We suggest writing:

"Over land there was greater disagreement between PlioMIP1 models and data than over the oceans, however uncertainties over land are also greater. Data sites considered by Salzmann et al. (2013) showed uncertainty due to bioclimate range between 0.5degC and 5.8degC, and dating uncertainty of up to 4.0degC. A modelled range of values was also considered which accounted for variability within the modelled ensemble, CO2 uncertainty and orbital uncertainty. Including all of these sources of uncertainty allowed models and data to overlap in many places, however the full range of uncertainties was large, meaning it would be difficult to determining the 'true' temperature. Also, there were still locations where model and data did not agree within the range of the uncertainties. At these locations Salzmann et al. (2013) noted that "the underlying reasons for these large and statistically significant DMC mismatches are unknown".

- *Line 73-74: the number of sites able to be directly compared to the models for KM5c is further reduced when a higher threshold for age control was introduced by the Pliovar*

working group (McClymont et al., 2020, Climate of the Past), which is worth pointing out here as the authors also use that data in Figure 1 and so line 80-81 seems to be referring it. I don't think the Pliovar data was used in the PlioMIP2 DMC described in line 73-74.

The reviewer is correct. The PlioMIP2 DMC described in lines 73-74 did not use the Pliovar data. We will change the text to:

"This meant that the 100 ocean sites that were included in a DMC for PlioMIP1 (Dowsett et al., 2013), had reduced to 32 ocean sites for PlioMIP2 (Figure1).

- *Line 82-83: "high latitude" here is really "high northern latitude" because there is only a single data point for the entire southern hemisphere, and it has already been described as aligning well with the models (line 82)*

In the revised version we will change this to high northern latitude as suggested

- l*ine 89: the authors could also cite the Pliovar comparison of McClymont (2020) since that also showed better model-data agreement? Aligning with the theme of this manuscript, the Pliovar paper includes a comparison of high northern latitude data and different monthly model outputs, which also indicates a possible role for seasonality in the oceans as well as on land.*

As suggested we will also cite McClymont et al 2020 near line 89 in the revised version.

- *line 169-170: data-model agreement for the PI and "no inherent model bias" – but there seems to be a tendency for the PI MMM (blue dots, Figure 2) to sit either at the low or high end of the PI data (blue dashed lines), which is not noted. Is there a reason for this tendency in the data or models?*

We think that the way we have presented this figure might be a bit misleading. The blue dashed line is the difference between CRU reanalysis data (representing a gridbox) and the point based observations. We have incorrectly labelled this in the figure as 'PI data uncertainty'.

Since model results are also gridbox based we would expect the difference between model and point based observation to be similar to that between CRU and point based observation, so the results do suggest "no inherent model bias".

In the revised version of the paper we will replace the blue dashed line with a blue star. We will also change the labelling in the legend to CRU 1901-1930. We will change the x-axis label to 'Difference between modelled/reanalysis and observed/reconstructed temperatures (degC)".

- *line 202: is "prior to 3.5 Ma" late Pliocene as this block is to indicate? How is early/late Pliocene being defined here?*

This is an error. In the revised version of the paper we will move the Lake Baikal site to the Early Pliocene block and move the corresponding text. We will also reorder table 3 and correct the 'Tnekveem and Hydzhak sites which should be labelled as Early Pliocene'

- *line 217-218 and some of the preceding sections where DMC is described: I didn't see it defined anywhere, but do the authors consider "agree reasonably well" or to have "similar outputs" between data and models to be the place where there is overlap between the spread of model outputs and the data "points"? There are some places where there is quite a large difference in MMM and data points (e.g. Mirny late Pliocene*

Figure 3) but where there is overlap in the range of model outputs – does this count as being "similar"?

For the purposes of this paper, it is non-trivial to define quantitatively what is meant by model and data agreeing reasonably well. This is because there are many site-dependant known errors that need accounting for in DMC. We wrote that model and data agreed "reasonably well" because we thought it was useful descriptive language for figure 3; however, what we really wanted to say was that model-data discrepancies for the warm month temperature are very small compared to those for the cold month temperature.

We suggest rewriting lines 217-218 as follows:

"Regardless of the exact dating, location or reconstruction method, the DMC over North America follows the same pattern as that seen over North Asia: the modelled temperatures are far too cold for the coldest month, and model-data agreement is much better for the warmest month.

- *Line 274: and cite the pliovar DMC?*

Agreed we will cite Pliovar here

- *Figure 6: I became confused here by the KM5c data-model anomalies, because the authors comment that only two sites (the top two) have KM5c data. So, is the KM5c "anomaly" for Mimy and all of the sites beneath this actually "difference between KM5c model and late Pliocene data"? Some revised text in the caption to clarify what these plots represent would help a lot, because at the moment I think it implies that there is KM5c data at all of the sites shown.*

Sorry for the confusion. We will make clear that the data is Late Pliocene for Mimy and sites below this.