Answers to: EC1

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Dear editor,

Many thanks for considering our manuscript for publication at WCD and taking the time to act as editor. We acknowledge your critiques and constructive suggestions for improving the manuscript. In this reply we will address your questions and respond to your suggestions.

"This paper presents EOFs of January NH midlatitude circulation variability in the Pliocene from the output of a set of previously published experiments from one model used in the PlioMIP2 project. The authors conclude the Pliocene climate is not an analog for a future climate under increasing CO2 because the variability in NH January is different because of differences in boundary conditions (orography) during the Pliocene vs. modern, **underlining the results from Menemenlis et al. (2021) who showed the Northern Hemisphere stationary wave is greatly reduced in the same model when late-Pliocene boundary conditions are used in place of modern day boundary conditions."**

We want to make a few clarifying remarks considering this sentence in the first paragraph:

- Although we agree some of our results underline those presented by Menemenlis et al (2021), we treat SLP and jet stream **variability**, while they are not, and mainly considering long-term means.
- The models are indeed very similar (essentially both adaptations of CCSM4), but differ in some important ways: different ocean mixing parameterisations, different coupling between the atm-ocn-ice models, and a different initialisation. Haywood et al (2020) discuss general features of all PlioMIP2 models and it shows that CCSM4-UoT (Menemenlis et al 2021) and CCSM4-Utr (present manuscript & Baatsen et al 2022) have quite different responses in the mean: https://cp.copernicus.org/articles/16/2095/2020/cp-16-2095-2020.pdf

Then, in response to your enumerated items:

"1. The manuscript falls short, however, in providing a dynamical analysis of why the variability and the mean state) changes under Pliocene boundary conditions. Further analysis should be done to demonstrate why the variance in the various patterns changes. For example, why does the variance in the PNA change? Is it due to a reduction in the mean state stationary wave – the main source of energy for the PNA (see, e.g., the discussion on page 237 of Wallace et al. 2023) – but a dynamical analysis should be performed to confirm this. Or is it due to a reduction in ENSO variability? The authors should quantify the different contributions to the change in variance of the PNA. Similarly, evidence through analysis should be provided on why the variance in the NPO changes."

1. **Dynamical analysis:**
   We agree that the dynamical analysis in the current manuscript, aiming to connect changes in variability to changes in the mean state and the boundary conditions, can be improved. We propose to include the following:
   a. Results of a measure that indicates how stationary waves change in the North Pacific. This could be the eddy streamfunction (as in Mememenlis et al), or

Rossby wave source, or another variable that is relevant. This can inform us whether a change in stationary waves is at the cause of changes in SLP variability.

b. Additionally, if our intended analysis at a. does not already provide enough detail, we will include more results on the mean dynamical state of the atmosphere; where we include a measure of velocity potential, (potential) temperature, and/or isentropic density. This can help us to explain whether a change in distribution of mass in the atmosphere is at the cause of changes in the mean jet and jet variability. Both results at a. and b. can be included in the Supplement with reference in the main paper.

c. Changes in ENSO amplitude (e.g. Nino3.4 SD) for all simulations. We know ENSO amplitude changes in the Eoi400 (see f.e. Baatsen et al 2022 or Oldeman et al 2021) but have not included a quantification for Eoi280. We will compute it and include the results, with which we aim to answer why SLP variability is reduced (instead of speculating in the Discussion). A more detailed analysis on ENSO is outside of the scope of this paper, but will be treated in planned future work.

"2. The discussion of why the surface air temperature changes in response to changing boundary conditions is speculative: without a quantitative analysis of the thermodynamic energy budget, one can't discern the relative importance of changes in the mean state stationary wave vs rectified effects of changes in the (PNA) transients. The changes in the mean state circulation would probably create a pattern of warming/cooling in the N. Pacific that is very similar to that in Fig. 3c, but it isn't clear to me that this pattern could result from changes in the variability in the PNA (as is argued in section 4.2). To support this claim, the revised paper should show the rectified effect of changing PNA variance and a quantitative analysis of the relative contributions of the mean state and transient changes to the thermodynamic balance warming tendency (e.g., calculate the changes in d($\nabla \cdot \boldsymbol{v}'T'$), δ($\nabla \cdot \boldsymbol{v}\ T$), ($\nabla \cdot \boldsymbol{Q}$), etc, where the overbar denotes time mean and the prime denotes transients)."

2. **SAT changes to boundary conditions**:
   a. First, we acknowledge that we cannot discern relative effects of all individual BCs (just gateways effect or just GIS effects) with our simulation suite. Hence, we refer to other studies where more specific sensitivity experiments have been performed.
   b. Then, to answer why we see certain surface temperature changes, we will include results of an energy budget analysis. More specifically, we propose to use an energy balance model similar to the one employed in Baatsen et al 2022 and Burton et al 2023. It would essentially be an extension of Fig 11 in Baatsen et al 2022, but then in winter (instead of annual mean) as well as per grid point (instead of zonal mean), so we can discern the contribution in different regions. This will tell us which contribution to the energy balance generates the warming (for example albedo effects or cloud effects), and we can use that information to distinguish the warming processes in E560 and Eoi280.
   c. Your suggestion to compute the thermodynamic balance warming tendency is appreciated. However, we are not convinced that such a calculation is necessary to distinguish the SAT changes in the different simulations. Indeed,

it would be interesting and could add valuable information, but we consider this to be outside the scope of the current manuscript.

"3. Concerning the changes in the mean state, Menemenlis et al. (2021) documented that the Northern Hemisphere stationary wave is greatly reduced in this model when late-Pliocene boundary conditions are used in place of modern day boundary conditions. Here, the authors speculate (using the results in section 4.2 of Hurwitz et al) that SLP increases in the Aleutian low in the Pliocene because of increases in SST in the N. Pacific. It is difficult to say for sure (because of the lack of contours and/or the poor resolution in the color bar used in Fig. 3c and other figures) but I don't think the scaling works. Hurwitz et al show a 30 m geopotential response at 850 hPa for a 2C warming in the N. Pacific, which amounts to approximate 3 hPa SLP response (=30 m *(hPa / 8 m) * 850/1000) for a 2C anomaly, or 1.5 hPa per 1 C anomaly. In response to Pliocene orography, there is a 16 hPa increase in the Aleutian Low and a ~4 C increase in N. Pacific SST (Figs. 2c and 3c), which is almost three times greater than the response to the prescribed SST anomalies. Indeed, the SST anomalies seem to be a response to the changes in the stationary wave, not the other way around."

3. **Hurwitz et al hypothesis:**
    a. We appreciate your effort at falsifying our proposed hypothesis / speculation. Your rough estimation indeed seems to point out that our comparison with the results by Hurwitz et al is not entirely valid. We propose to remove the section in the Discussion where we make this comparison. Alternatively, we can keep the reference, but mention that the comparison might not be entirely valid, as your estimation shows.
    b. Regarding "lack of contours and/or poor resolution", we will change our continuous colormap to a discrete colormap. In this way, contours will be included, and it will be easier to distinguish specific values.

"4. Another, more likely, cause of the stationary wave response to Pliocene boundary conditions is changes in the tropical Pacific diabatic heating (precipitation). There is a long literature dating back to Simmons et al. (1983) that shows the strength of the Aleutian low and the amplitude of the stationary wave is sensitive to small changes in diabatic heating over the Maritime continent. Figure 1 of Menemenlis et al. (2021) shows that, in response to Pliocene boundary condition, precipitation is reduced over the far western Pacific and increased in the (unrealistic) double ITCZ in central and eastern Pacific. Hence, it would seem changes in the tropical Pacific climatology could easily be responsible for the changes in the climatological mean state Aleutian Low and the stationary wave (at least, in the Pacific), for the weakening and broadening of the climatological mean jet, and for the changes in the variability in the PNA. Simple AMIP experiments using prescribed climatological SSTs taken from the E280 and Eoi280 simulations would illuminate the cause(s) for these changes in the simulations."

4. **Tropical Pacific heating:**
    a. Thank you for this comment. We believe that with including a measure on stationary waves (suggested at 1a.) as well as the energy budget in the Pacific (suggested at 2b.), we will already have some parts available to answer this question. Additionally, we propose to extend our SAT results to lower latitudes, for example 20S, to include the tropics. On top of that, we will include precipitation results in the Supplement, or alternatively as contours on the SAT or SLP results. (Fig 2 or 3).

b. The suggested AMIP experiments could indeed illuminate the links between tropical heating and the atmospheric response. However, we think that by assessing the tropical Pacific climatology (as outlined in a.) as well as the connection with ENSO (as outlined in 1c.) in the simulations currently treated in the manuscript, we can cover the role of the tropical Pacific in North Pacific variability. Therefore, we do not think that additional AMIP experiments are necessary for answering our research questions, and so we consider them to be outside of the scope of this paper.

c. To add to the last answer, we have however performed an additional fully coupled experiment, the Eoi560 (in addition to: E280, E560, Eoi280, Eoi400 that are already treated in the current manuscript), which is a 2x pre-industrial CO2 simulation with mid-Pliocene boundary conditions. We share more detail on this simulation at answer 9b.

“5. January and February are special months in the N. Pacific when the jet takes on a more subtropical location and becomes strong and supports less variability – the so-called Pacific mid-winter suppression of the jet. I am not surprised that the EOFs of DJF circulation change in a similar in the Pliocene to those shown in the paper for January (but showing that analysis instead of the analysis of January only would boost the statistical significance of the results). Perhaps even more interesting, it is less clear the other winter months – ONDM, the stormy months in the Pacific – will show the same Pliocene minus modern differences as those in the mid-winter suppression months. Streamlining the introduction and discussion of previous results concerning mean state changes and removing tangential discussion on changes in heat transport in section 4.2 would leave room for a comparison of the changes in variability.”

5. **Variability in (extended) winter months:**
    a. Thank you for this useful remark. Regarding the increased statistical significance of DJF over January-only results, we propose to repeat the performed analyses for DJF instead of January only. We will include some January-only results, or even extended winter (NDJFM), in the Supplement, to make the comparison between January-only results and DJF results quantitative.
    b. Streamlining introduction and discussion: We will change the Introduction, as well as better streamline the Discussion, as has been outlined in the answers to Reviewer 1 and 2.
    c. Ultimately, we think that mid-winter suppression is interesting, but not necessary to treat in order to answer our research questions. Thus, we regard the assessment of Pacific mid-winter suppression of the jet to be outside of the scope of the current manuscript.

“6. Consider analyzing the variability and mean state changes in at least one other climate model used in the PlioMIP2 project. Are your results sensitive to the model used? Fig 1a of the paper shows that the biases in the modern day January stationary wave in the model are large – about twice too large in the N. Atlantic and 40% too large in the N. Pacific – and so too is the variability too large – by a factor of 2 or three.”

6. **Repeat analysis in another model**
    a. Yes, we expect that the results are sensitive to the model used. This is based on the fact that the PlioMIP2 models show a range of mean state responses (e.g. Haywood et al 2020), different sensitivity to CO_2 (e.g. Burton et al 2023),

as well as a range of ENSO responses (Oldeman et al 2021 and Pontes et al 2022). We discuss these results briefly in the Introduction, but we currently do not treat the model dependence in the Discussion. We will include a section in the Discussion where we mention this.

b. Regarding comparison to reanalysis: the SLP biases are locally large, especially in MSLP and SLP variance (less so in the EOFs). We will explicitly mention the implication of these biases in the Discussion.

c. Although we can expect differences with different models, we do not think it to be necessary to repeat analyses with another model. Model dependency regarding Pacific variability in the midPliocene will be treated in planned future work.

"7. The use of nonstandard (and apparently arbitrary) assignments of the labels "zonal" and "azonal" terminology to describe well know patterns of atmospheric variability is needlessly confusing. Without further justification, I strongly urge the authors to use standard monikers for these patterns to avoid needlessly confusing the readers. [E.g., the NAO and NPO describe regional-scale patterns of variability featuring meridional dipoles in geopotential, changes in the jet strength, and changes in the meridional location of the storm track. It is difficult to see how that fits with the monikers "zonal" and "azonal".]"

7. **Zonal / azonal terminology**
    a. Thank you for this remark, which echoes the comments of reviewers 1 and 2. So, we will change it accordingly, as we have also mentioned in more detail in the answers to reviewers 1 and 2.

"8. Consider using ERA5 instead of CR20 for the modern "observations", or truncate the CR20 period to start in the early-mid 1900s. The former has 72 years of very good data; the latter is less constrained – especially in the first half of the analysis period used (1836-2015)."

8. **Use ERA5 instead of CR20**
    a. We specifically looked for a reanalysis dataset that would be well suited to compare to our equilibrated pre-industrial simulation results (E280). We considered ERA5, because of its high quality, but decided not to use it since it only spans a short period (relative to our 200 years) and it is a better representation of the present-day (instead of pre-industrial). Instead, we chose to use CR20, exactly for the reason that it covers more of the 'pre-industrial' period (although one can argue that even 1836 is not pre-industrial), and because of the length of the dataset (179 years). Furthermore, the spatial and temporal resolution of the CR20 is still sufficient for comparison to our pre-industrial results. We believe that using CR20 over ERA5 is justified.

"9. I agree with both reviewers that the title doesn't fit the contents of the paper (e.g., the title refers to generic warm climates rather than the late Pliocene) and that adding an analysis of the response to an increase in CO2 under late Pliocene conditions (the change in the pair of experiments Eoi400 and Eoi280) would add new results to the paper (vis à vis the response to increased CO2 under different boundary conditions)."

9. **Title and extra simulation results**
    a. Regarding the title: we agree with you, and both reviewers, that an alternative title would be better. We propose the following title (see also the

reviewer answers): “**Mid-Pliocene not analoguous to high CO_2 climate when considering Northern Hemisphere winter variability**”

b. Regarding analysis of response to increased CO_2 under Pliocene boundary conditions: we agree with you, and both reviewers, that this would be interesting to include as well as relevant in answering our research question. We have performed a ‘CO2 doubling’ experiment in the mid-Pliocene, the Eoi560 (see Baatsen et al 2022). The results of this simulation can be used to compare to the CO2 doubling with pre-industrial BCs (i.e. Eoi560-Eoi280 versus E560-E280). In addition, it can be used to compare to the mid-Pliocene BC effects at high CO2 (i.e. Eoi560-E560 versus Eoi280-E280). We chose not to include the results of this simulation in the present manuscript, as we figured it would not be necessary to answer the research questions. We propose to include some Eoi560 results in the revised manuscript. See also more detail on this in the answers to reviewers 1 and 2.