

Answers to: RC2

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Review of: Past warm climate conditions show a shift in Northern Hemisphere winter variability towards a dominant North Pacific Oscillation (Oldeman et al., 2023)

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Dear anonymous reviewer 2,

First of all, many thanks for considering our manuscript for publication and taking the time to review. We appreciate your positive words, and we acknowledge your critiques and constructive feedback. In this document we will aim to answer your questions and respond to your feedback, in a way that we hope to be satisfying. Ultimately, we think the manuscript will be improved and will be accepted for publication after intended revisions.

In this study the authors pose the question of whether the mid-Pliocene can act as a suitable analog for future climate change scenarios given the relatively similar CO₂ concentrations and geography to present day. In particular they focus on suitability for studies of NH winter variability. They use a suite of simulations from CESM1.0.5, part of PlioMIP2, to separate the climate response to CO₂ doubling vs. mid-Pliocene boundary conditions. A major finding of this manuscript is that the dominant mode of North Pacific variability differs depending on whether we consider CO₂ doubling or mid-Pliocene boundary conditions: the preindustrial dominant mode (the PNA) strengthens under climate change whereas under mid-Pliocene boundary conditions the NPO becomes the dominant mode. The authors conclude by saying that the mid-Pliocene is not a suitable analog for future climate change.

I thank the authors for an interesting study. While the result that different boundary conditions lead to different climate variability is not necessarily shocking, it's valuable to explain that the North Pacific variability in particular changes depending on the forcing. I have some questions and suggestions that I hope will be addressed before publication. I hope the authors find the comments below constructive and useful and I look forward to reading their responses.

Overarching Comments

1. General Motivation: Overall I found the general motivation of this paper to be somewhat unclear. After requisite background on NH wintertime variability and why the mid-Pliocene is considered a potential analog for future climate change, the paragraph beginning at L61 seems to lay out the main question of this study well. The authors then proceed to cite studies which say the two periods are not good analogs for each other. My question, then, is where does the current study fit in? Do prior studies not discuss wintertime NH variability between the periods? Does this study start from the hypothesis that the two periods will be not agree well but the authors seek to quantify that hypothesis? As you clarify this, the discussion at L417 (and throughout Sec 4.1) might become more suited for the introduction.

Thank you for this remark. We see that the current structure of the Introduction might be confusing, and a different structure and ordering can help to put the results and discussion in better context. We propose to restructure the Introduction (also to answer a main critique by Reviewer1), and introduce subquestions to the main research question, a hypothesis based on previous work (to clarify “why does this study fit in?”) and move some literature introduced in Discussion 4.1 to the Introduction.

Proposal of paragraphs of the Introduction (for more context see also the answers to RC1):

1. Introduce the midPliocene as most recent period with atmospheric CO₂ similar to present-day. Explain why midPliocene is considered a possible ‘best analog’ for near-future climate (when considering mean temperature and precipitation and compared to RCP4.5 projections, see Burke et al 2018) – would include parts of current L42-51
2. Introduce midPliocene modelling efforts – would include current L52-60
3. Explain midPliocene can also be relevant for long term climate projections, in terms of CO₂ (400ppm similar to optimistic SSP1-2.6 scenario in 2300), or when considering reduced Greenland ice sheet and West Antarctic ice sheet. – would include some parts and literature that is now presented in Discussion 4.4 The mid-Pliocene as future analog?
4. Explain next to CO₂ and reduced GIS, there were also closed gateways, which previous studies have shown to have effect on mean climate – move some literature and sentences from Discussion section 4.1 Sensitivity to mid-Pliocene boundary conditions here
5. Apart from mean climate, we can also study climate variability in the past, in order to further understand climate dynamics & variability response in warm climates, as well as how mean climate changes relate to changes in variability - Cite work on midPliocene ENSO, a.o.
6. Also very relevant to study winter variability, such as NAO. Large impacts, but future projections not in agreement. What happens with winter variability in warmer climates? – This would largely be L19-41
7. Brings us to the rephrased research question: “Can the mid-Pliocene be used to investigate the response of NH winter variability to a warmer climate?” with

subquestions: 1. Is there a difference in the response to elevated CO₂, and to mid-Pliocene boundary conditions other than CO₂, including closed Arctic gateways and reduced ice sheets? And 2. How do changes in mean winter climate relate to changes in atmospheric variability?

8. Introduce the model used and the specific simulations used to answer the questions – corresponding to L87-L95
 9. Briefly hypothesize what we expect the answers to be based on previous studies – this would consist of parts from L61-L76, section 4.1, and section 4.3
 10. Paper outline – current L96-100
2. Results Section: As it stands, Section 3 reads as if the authors made a list of plots and then simply describe them to the reader. Instead of this, I recommend the authors consider their primary motivation (see above) and then present their results in a way that strengthens their argument and presents a coherent storyline rather than just marching through mean SLP, SST, and U200, then std SLP, SST, and U200, and then figures about the jet. For instance, if the hypothesis is that the two eras won't agree well, weave in interpretation of what you're seeing in the difference between the b and c panels of Figs 2-4. Explain why differences in the EOFs lead you to consider the jet, how the differences in surface forcing impact the jet and weather, etc. Interpretation is not inappropriate in the Results section and will lead to a stronger article.

We agree the Results section is quite descriptive and could use more interpretation. This was a choice we made, but we understand it doesn't read well and leaves interpretation to the reader, which is not the intent. We propose the following:

- We will reduce the level of description in the Results, in those places where the description is not really necessary for answering the research questions, or where description is redundant because it is obvious from the presented Figures.
- We will increase interpretation in the Results in two ways.
 - o First is to move some of the interpretation that is currently presented in the Discussion, specifically Section 4.2 Physical and dynamical interpretation, to the parts in the Results where it would fit. For example L438-445, a paragraph offering interpretation of the CO₂ doubling results and making a link between the mean changes and variability changes, can almost entirely be moved to the Results section 3.2.1 (Sea-level pressure variability / CO₂ doubling).
 - o Secondly we intend to include subquestions to the main research question in the Introduction (including: "How do changes in the mean winter state relate to changes in winter atmospheric variability?") as well as some hypothesis based on literature that is now newly presented in the Discussion (mainly section 4.1). In the Results section, when presenting our results we can then refer back to the questions posed in the introduction, as well as to our initial hypothesis. For example, linking mean state changes to changing in variability is what we are doing in the previously mentioned paragraph L438-445. Moving that to the Results, and referring to the question asked, will hopefully increase the level of readability of our Results section.

3. Section 4.2: I had a hard time understanding which dynamical interpretations were from past studies and which were from the figures in this paper. I especially was confused at the “three major aspects” section. For instance, atmospheric heat transport was not discussed elsewhere in this paper but makes an appearance here. I recommend the authors clearly outline where these dynamical interpretations come from their own results and where they are pulling from prior studies. It seems like a lot of this section results from prior studies, which makes me wonder how much of it should be in the introduction.

We agree that our Discussion is currently introducing some literature that was not treated earlier in the paper (for example, in the Introduction). Next to that, how the Discussion is currently written can indeed be confusing as to when we refer to our own results, and when to previously published results. We propose the following:

- We intend to reorder the Introduction (as outlined above), where we will introduce a paragraph on the effects of the different boundary conditions of the Pliocene on the climate. This would include parts of section 4.1 Sensitivity to mid-Pliocene boundary conditions. For example paragraph L417-424 is treating SAT and SIE response to different mid-Pliocene boundary conditions in other climate models, which would actually be relevant to mention in the Introduction.
 - We will rephrase some sentences and restructure some parts of the Discussion in such a way that it is clear what is our results or our analysis, and what are findings from previous studies. For example L491-492 (“We observe ... Arctic gateways (refs).”); the first part of this sentence is about our results, while the second part refers to findings in previous studies. A proposed rephrasing would be: “Qualitatively the loss of winter sea ice is similar for the Eoi280 and E560 (see Fig 3). However, based on previous research (refs) we know that mid-Pliocene sea-ice loss in the North Pacific is caused by the closed Arctic gateways.”
4. Title: I have two thoughts about the title, the second of which relates to Overarching Comment #1. First, I think “warm climate conditions” is too general; the authors only focus on the mid-Pliocene. Second, the title doesn’t really seem to capture the main point of the paper. Although the increased dominance of the NPO over the PNA is an interesting result, it seems to me that the main point of this paper is that the mid-Pliocene is a bad analog for future climate change when studying NH winter variability. The authors say this specifically in L13-14 of the abstract.

Thank you, we agree the title can be potentially misleading. The other reviewer also has a comment on the title, so to answer to both, we propose the following title:

“Mid-Pliocene not analogous to high CO₂ climate when considering Northern Hemisphere winter variability”

5. Zonality & Azonality: At L185 the concepts of zonality and azonality are introduced and for the rest of the paper I proceeded to get confused about which was which. To begin, I don’t think that “azonal” is a common word—for instance, there is no

definition in the AMS glossary. I think this a good first check on whether a term needs to be defined. How do the authors define azonal specifically? Presumably they mean something different than “meridional,” correct? How do they decide which mode is zonal and which is azonal? Eyeballing? Second, when I think of the NAO and the NPO, I generally think of them as meridional modes of variability with a center of action in the subtropics and then the other center of action directly above the first in the subpolar region. The authors however say “The NAO is essentially the zonal mode in the NATl” in L185-186. The PNA, which I think of as a zonal mode with a center of action over the Aleutian Low and one directly eastward over North America, is then identified as the azonal mode. Please explain your reasoning for these descriptions as they seem to be the opposite of my intuition. Last, I found the NPac-z/NPac-a/NAtl-z/NAtl-a terminology confusing, partially because of the confusion around zonal and azonal mentioned before. Why not just say the NPO mode, the PNA mode, the NAO mode, and the EA modes? In the titles of the figures, you say e.g. “NAtl-z (NAO)”, so why not just cut out the strange labels and make it so the readers have four less things to keep track of?

Thank you for your comment on the nomenclature used in this paper. It was a choice not to call the NAO the NAO, since we are not sure if the NAO in the midPliocene is the same.

We determine level of zonality by subjectively looking / eyeballing. We have employed a method earlier that used the same definitions used for changing sign of the EOF (L188-190), but this still required a lot of (subjective) tuning so that it worked well. In the end, we are just dealing with two times four EOFs per basin (CR20, E280, Eoi280, E560), so we chose to determine the level of zonality qualitatively.

Furthermore, we propose to stick to the known nomenclature (NAO, PNA, etc) instead of NATl-z, NPac-a, in order to answer to your remark, as well as a remark from reviewer 1. We agree that the current naming convention, including the definition of ‘zonality’, might confuse the reader. Sticking to known nomenclature will help, with the side note that the modes in the midPliocene might not be exactly what we expect from the present-day. This is for example also what has been done in previous studies regarding NAO in past climate (e.g. the LGM, see <https://journals.ametsoc.org/view/journals/clim/23/11/2010jcli3372.1.xml>)

6. Simulation Permutations: It might be informative to show Eoi400-Eoi280 differences. While this isn’t precisely the equivalent for comparison with E560-E280 since it’s not a doubling, this difference can help us understand whether increased CO₂ with mid-Pliocene BCs is similar or different in pattern. This might help with interpretation in the discussion, though it doesn’t seem like you have enough simulations to explore the full space of nonlinearity. I think E400 would be needed for that.

Thank you for this suggestion. We have actually 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, as we figured it would not be necessary to answer the research questions.

However, to answer your point as well as reviewer 1, we propose to include some Eoi560 results in the revised manuscript.

We propose to discuss the Eoi400 vs E280 results in more detail in the Discussion, in the section where we currently discuss nonlinearity (L499-506). Specifically, we propose to show a set of figures in the Supplement that we will then refer to, namely the MSLP difference and SLP SD difference for the following five combination (i.e. 5 panels per variable):

1. E560 – E280 (effect of CO2 doubling)
2. Eoi560 – Eoi280 (effect of CO2 doubling with mid-Pliocene BCs)
3. Eoi280 – E280 (effect of mid-Pliocene BCs)
4. Eoi560 – E560 (effect of mid-Pliocene BCs at high CO2)
5. Eoi400 – E280 ('true' mid-Pliocene conditions vs pre-industrial reference)

With this information, we believe that we can properly address nonlinearity, as well as the combined effects of CO2 and other mid-Pliocene BCs in the 'true' mid-Pliocene Eoi400 simulations.

Substantive Comments

1. L19-21: The statement that future climate projections fail to give consistent responses to greenhouse forcing among NH winter variability modes merits a citation.

Later, we cite the IPCC AR6 WGI report to support this claim (L26-27 "in particular because there is no consensus on how the NAM and NAO respond to increasing CO2 concentrations (Eyring et al., 2021)"). We will include that citation in L19-21 as well.

2. L77-87: This paragraph seems out of place. You don't really touch on proxy reconstructions in this paper, just modeled results.

Agreed. We introduced this section to discuss the fact that comparing with proxy reconstructions for this topic would be hard, if not impossible, because of the lack of atmospheric proxies of the midPliocene. But currently, as well as in the newly proposed Introduction structure, the paragraph is not necessary. We propose to move some of the sentences to parts in the Results or Discussion where we interpret and discuss the results.

3. 1.3: I'm confused about what data was previously available and which simulations the authors have run for this study. Some clarification is needed. Have you run these simulations of E560 yourselves? L125 suggests you're using this model specifically because other sensitivity experiments are available.

We understand the confusion. Most simulations had already been performed and have been published in Baatsen et al. (2022). Only the E560 used here is a slight adaptation of the E560 published in Baatsen et al; we treat that in L153-159. To clarify: the published E560

simulation was continued with a minor difference in parametrization that was found to give no influential differences in the E280 case. We will clarify as follows: replace “we perform ..” with “we use” or “we employ ..”. Also, we will add a sentence stating: “The model specifics and simulations employed in this study have been described in Baatsen et al (2022) and have been run for longer times for use in this study (see spin in Figure S1).”

4. L181 and throughout: NHem is not the universally accepted abbreviation for Northern Hemisphere. I very strongly recommend changing it to the common “NH.”

Thank you; we used it to indicate the 1st EOF in the Northern Hemisphere, but since we will adopt the usual terms (either NAM or AO) we can skip using NHem, and will use NH when we mean the Northern Hemisphere.

5. L209: “The mean MSLP difference is very small...” → are you talking about some area mean value?

Yes, we mean spatial mean MSLP. However, it might not be particularly meaningful (see also answer to reviewer 1). We propose to remove this sentence altogether, and instead compute RMSE (computed per grid point, then averaged).

6. Fig 1, 5, 6: Dashed vs. dash-dotted lines are difficult to discern. I recommend solid for positive, dashed for negative, and thicker line for 0.

We chose to avoid using solid lines in the contour plots, because it was a bit confusing with the coastlines. However, both reviewers have a comment on the readability of the contour lines, so we will reconsider. Your suggestion is appreciated and we will try that.

7. L218: You already defined your acronyms in L181-182. You then redefine them again in L295.

Thank you, it is indeed unnecessary, we will remove here.

8. Fig 2, 3, 4, 6 Captions: Write “minus” or use a minus sign rather than “min” as it currently stands. It’s only two more letters and you use “min” at other points to mean minimum.

Thanks for the suggestion, we will use ‘minus’ in the caption and other places in text where we indicate the difference. (or; write ‘difference between x and y’)

9. L263: An arctic amplification citation would be useful here.

OK, we propose to cite Serreze & Barry 2011 with a synthesis on Arctic amplification research: <https://www.sciencedirect.com/science/article/abs/pii/S0921818111000397>

10. L283: “a lot stronger” — this seems arbitrary. I recommend quantifying how much stronger. Similarly, you use “a lot stronger” again in L308 and “a lot weaker” in L513.

We will try to avoid these arbitrary or qualitative descriptions as much as possible. In L283, we propose “.. a lot more obvious ..”. In L308 we actually quantify the results already. In L518 we will include a quantification.

11. L284: “significantly” -- do you mean statistically significantly or just substantially weaker?

We mean substantially and will use that word instead (no specific relevance for introducing statistical significance here).

In other cases where we compute the Pearson correlation we will introduce statistical significance (e.g. $p\text{-value} < 0.05$).

12. Fig 8: It seems like we’re missing some panels here. Unless I’m misreading this figure, you don’t show the correlation of jet intensity with NPac-z or jet latitude with NPac-a. Why is that? I recommend adding those panels. Also, it would be worth mentioning somewhere that the Eoi280 scatter in Fig 8d is nonlinear so the linear correlation might not be the best metric.

1. Agreed the ‘opposite’ scatters and correlations might be interesting, however they are not as relevant / pronounced. We will add this in the supplement and mention the results in the main text with ref to Suppl.

2. We will also add statistical significance here instead of the R^2

3. In L375-377 we mention that the distribution is not unimodal, has two peaks corresponding to the two-jet state in one of the phases. However, we agree it is not explicitly mentioned that the relation might be nonlinear. We will add a sentence “The fact that a split jet exists in the NPO- phase implies that the distribution of jet latitudes is not linear, which suggests that a linear fit might not be the best metric to capture the correlation.”

13. L403-405: Citation for split jet and wave breaking?

We propose to include the following relevant studies on wave breaking and modes of winter variability: <https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2010GL043309> and <https://journals.ametsoc.org/view/journals/clim/23/11/2010jcli3372.1.xml>

14. L505-506: Different simulation permutations may help with disentangling the effects of BCs vs. CO_2 . See Overarching Comment #6.

See our earlier answer on the Eoi560 simulation and nonlinearity.

15. The capitalizations throughout the citations seem to be somewhat arbitrary and using different citation styles. I suggest standardizing to one citation style.

Thank you, we will standardize this.

Minor Typos

- L1: "...we address the question OF whether..." OK, will change this.
- L16-17: "...there is a need to make..." -- avoid the passive voice as able Propose to change into: "... and accurate projections of our future climate is necessary."
- L106: "Next to that..." -- doesn't make sense in this sentence We will remove it
- L114: The community seems to refer to it as "CESM" not "The CESM" when using the acronym You are correct, we will remove "the"
- L117: Why "therefore"? This doesn't make sense to me. "therefore" in the sense that the PlioMIP2 community refers to the this version of CESM as CCSM4 because of its use of CAM4. However, the use of "therefore" is not really necessary and can confuse the reader, so we propose to remove it.
- L201: "It is not be a one-on-one comparison" -- incorrect wording Will remove "be"
- L368: "as well between both azonal modes" -- strange wording Will change this when changing the nomenclature of the modes.
- L381: UK vs. US English spelling is interspersed throughout, e.g., "behavior" here, "behaviour" in L172, 364, 464. I recommend sticking to one or the other. We propose to stick to UK English and will make spelling changes accordingly
- L440: Is STJ subtropical jet? This acronym was not introduced before this line. Yes, it is, and we will introduce it
- L525: "we posed the question OF whether" Will make the proposed change
- L527: Typo-- "analogous" Thank you, will change
- L529: "WHO state" Will change