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.

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.

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 $\delta(\nabla \cdot \overline{\boldsymbol{v}'T'})$, $\delta(\nabla \cdot \overline{\boldsymbol{v}}\,\overline{T})$, $(\nabla \cdot \overline{\boldsymbol{Q}})$, etc, where the overbar denotes time mean and the prime denotes transients).

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.

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.

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.

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.

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”.]

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).

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).

References not already included in the manuscript

Simmons, A. J., J. M. Wallace, and G. W. Branstator, 1983: Barotropic wave propagation and instability, and atmospheric teleconnection patterns. *Journal of the Atmospheric Sciences*, **40**, 1363–1392.

Wallace, J. M., D. S. Battisti, D. W. J. Thompson, and D. L. Hartmann, 2023: *The Atmospheric General Circulation*. Cambridge University Press, 424 pp.