Schemm and Röthlisberger developed a setup for aquaplanet experiments (APE), that incorporates hemispheric asymmetries (winter and summer hemisphere). They use this setup to study a range of behaviours and phenomena, mostly related to mid-latitude jet and eddy activity, in parameter regimes corresponding to "current" and "warmer" climates, as well as single and double ITCZ settings.

This is an overall interesting study nicely showing a sample of what can be done with rather simple models. Many of the aspects that are investigated relate to highly relevant and somewhat open research questions regarding climate change and general circulation behaviour. Key results include that previously reported increase in jet-waviness on a specific isentropic level in a warmer climate could be an artefact due to a vertical shift of that level and waviness at a fixed pressure might actually decrease, as well as the observation of small-scale wave weakening and large-scale waves strengthening during global warming, consistent with other studies.

The manuscript is fairly well written, the structure is clear and the figures are good. The scientific reasoning and development of conclusions seem sound. I have a few minor comments and questions for the authors, but can otherwise recommend the paper to be published.

We would like to thank the reviewer for the positive and thoughtful assessment of our manuscript. We will try to improve the reading flow when revising the manuscript.

General remarks:

- First, I have to admit that I am no expert studying APEs. The authors give information on typical APE setups (e.g. Neale and Hoskins, 2000), but I still struggle to fully understand the advantage of the hemispheric asymmetry in the "new" simulations. I feel like most of the analyses could have been performed in two separate symmetric runs (potentially with half the runtime even, keeping the computational costs equal). Is the suggested advantage simply to allow for asymmetric double ITCZ runs? In that case, I think some additional justification of why this (tropical) asymmetry should affect mid-latitude jet and eddy behaviour might be helpful. Again, maybe this only requires some further clarification.

Very good point. However, it is unlikely that this simulation would be possible in two symmetrical simulations. Symmetric winter runs are widely used. They are based on the zonal mean SST of the DJF seasonal mean in the NH, which is mirrored at the equator. Consequently, the maximum SST is at the equator and both hemispheres are symmetric. However, the summer SST used here is characterised by a poleward shift of the SST maximum. Mirroring the summer SST would result in two peaks at 5°N and 5°S and a local SST minimum at the equator, which does not seem ideal to us. The JJA SST is ideal for creating an aquaplanet with a summer and winter-like SST profile as it allows both hemispheres also to interact. A study that includes the interactions between the hemispheres (e.g., through cross-equatorial transport by the ITCZ) as the planet is warmed would not be possible with a pure summer or winter aquaplanet setup.

How the exact ITCZ structure affects the mid-latitudes is open to debate, but the subtropical jet lies along the edge of the ITCZ, its exact position is known to affect the mid-latitudes (see midwinter suppression), so one might initially expect an influence depending on the single and double ITCZ configuration. We wanted to present results for both cases, but our main finding is that the results are quite similar, which is an important take away message. From a modelling point of view, it is also very exciting that our setup allows us to control for the occurrence of both single and double ITCZs with very little change. The setup may prove useful in the future to test new model generations, for example at very high resolution, for tending to produce a single or double ITCZ.

- I think many of the figures could benefit from row and column labels. The information is mostly given in the caption, but this could help the reader take in the content.

We added row and column labels wherever we felt they were useful.

- Maybe I missed something, but I don't understand why you would expect any zonal asymmetries in terms of a preferred wave phase (Sec. 4.3)? Using purely zonally symmetric boundary conditions the mean response should (given sufficient statistics) also be perfectly symmetric, right?

  We agree with the reviewer that one does not expect a preferred phasing of high-amplitude waves in our APEs, which is confirmed by our results. The reason for nevertheless including these results are twofold: Firstly, we wanted to also include an analysis in this paper where circulation features observed in comprehensive GCMs/reanalyses are NOT reproduced by our APE simulations (contrary to the "large-get-stronger, small-get-weaker" response to warming, or the seasonal differences in the simulated storm track shifts). Secondly, even though this result could have clearly been anticipated based on basic atmospheric dynamics reasoning, the lack of a preferred phasing of high-amplitude synoptic wavenumber waves in our APE suggests that any such preferred phasing must have its physical causes in zonal asymmetries. We believe that for synoptic-wavenumber waves this point has not been made expicitly in previous literature, and our summer-like APE lends itself to underlining this in a simple and straightforward manner. For these reasons we feel it is justified to include this analysis in the paper, even though we fully agree with the reviewer that the result is not surprising. To make our initial hypothesis more explicit we now write at the beginning of Section 4.4 that we do not expect any preferred phasing of high-amplitude waves in our APE

Specific remarks:

L100: Please introduce the acronym APE

Done.

L134 "see below": Maybe better reference the specific section

We removed all "see below" statements.

Eq. 1: Since you are interested in the vertical structure, I was just wondering what happens if you normalise your Delta A_k with some density factor (or equivalently study changes in wave energy). Not sure if this leads to any insights or different conclusions, but might be worth considering.

Interesting comment, thank you! We believe your suggestion would indeed be the preferred approach if one was working with some form of wave energy or EKE (e.g., as in Chemke and Ming, 2020). Here, however, we investigate only the amplitude of different wavenumber waves in the meridional wind field. In our case weighting Delta A_k with density would not yield a quantity that is physically very easily interpretable. Also, note that the sign of the Delta A_k, which is essentially what we discuss as the "large-get-stronger, small-get-weaker wave response to warming", would not change when weighting by some density measure. For these reasons we refrain from incorporating your suggestion into this analysis, even though we fully acknowledge its relevance for related analyses of wave energy or EKE.

L148 "we first compute": The order doesn't really matter here, but ok.

We removed "first".

L152-153: Is this supposed to refer to A_k rather than Phi_hov,k? Otherwise, I don't understand what the condition means in terms of a phase. I it is about the amplitude, you could also move this paragraph to the previous subsection.

Yes, thank you very much for spotting this typo – of course we mean A_k!

L165: Remove "(southern)" as you mention SH blocks at the end of the paragraph

Ok, done.

L166: Not sure if the PVU explanation is needed, but if you want to include it I would rather move it to the end of the sentence or so.

PVU is an abbreviation and we thus want to define it where we first use it. We moved it to a footnote.

Eq. 4+5 and L228-229: You could consider to introduce separate parameters for NH and SH (like phi_0,N and phi_0,S or so). That would make the equations look a bit more complicated but might make the description of the overall setup a bit easier to understand.

We tried different variants but felt that the current version in combination with the two sentences following the equations remains our favorite.

Fig. 1: Could you motivate your choices of SST distribution? Later you distinguish between East and West Pacific, do you find your profiles to match these regions?

The first profile is motivated by the mean zonal SST of the JJA and leads to the occurrence of a double ITCZ. As an alternative, we want to create a profile that leads to a single ITCZ by increasing the equatorial SST gradient. They are not motivated by the SST profiles in the East and West Pacific and do not match these better than the zonal mean JJA SST profiles.

L280: The reference (Fig. 5a) seems to refer more to the previous sentence.

The reference was removed.

L290: You discuss Fig. 5 before Fig. 4, maybe swap them or change the discussion?

Indeed, we swapped them.

L338: *isentropic slope (to be more precise)

Thank you, corrected.

Fig. 7: What exactly does the "weighting" refer to? Also, please add sample sizes to the caption.

A weighted histogram is simply a histogram where the counts in each bin have been divided by the total number of counts, with the intention that the resulting histogram can be considered an estimate of an underlying probability density function and thus allows for comparisons across histograms based on different total counts. We now replaced the "weighted" with "normalised", as we feel that "normalised histogram" is a term that is sufficiently standard so that it can be used without further explanations. The sample size varies for each panel, but the range of sample sizes is now given in the caption of Fig. 7.