Weather Clim. Dynam. Discuss., referee comment RC2
https://doi.org/10.5194/wcd-2021-13-RC2, 2021

Comment on wcd-2021-13

Anonymous Referee #2

Referee comment on "Relative importance of tropopause structure and diabatic heating for baroclinic instability" by Kristine Flacké Haualand and Thomas Spengler, Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2021-13-RC2, 2021

The authors explore different representations of the tropopause transition in basic state variables and their role in baroclinic normal mode growth in an Eady-like setup including a stratosphere. Specifically, vertical shear and static stability are implemented either as step-like vertical profiles or with a smoother transition from tropospheric to stratospheric values. It is found that smoothing this tropopause transition in one or both of these variables has generally small effects on baroclinic growth. Furthermore, these effects are mostly non-trivial. What matters most appears to be the vertically integrated basic state meridional PV gradient, which depends to some degree on the specifics on the modified setups. These results are contrasted with the effects due to tropospheric diabatic heating, which are found to be much more important than the representation of tropopause sharpness.

The results are interesting and help to clarify questions related to misrepresentations of certain processes and structures in numerical models. Use of a highly idealized setup has the advantage of being able to allow quantitative mechanistic insights, but as usual comes at the price that it remains an open question how the results carry over to complex models and/or the real atmosphere. Overall the presentation is clear and I don't see any major objections to publication, although I do have some general as well as specific comments that I hope will help the authors improve their presentation and discussion of results.

General comments:

1. Comparison between the effects of tropopause sharpness and latent heating is useful. But I think a cleaner distinction would help: essentially, tropopause sharpness represents a modification of the already existing basic sate PV gradient structure whereas latent heating introduces new PV gradient structures. So even just intuitively it seems more

likely for the latter to have a more significant effect due to the stronger, more qualitative modification of the basic state. So, contrasting both effects one-to-one may be a bit "unfair".

2. Do you think the results on tropopause sharpness would change much for the case of non-zero interior PV gradient? One reason they might is that with the classic Eady setup the tropopause PV gradient always stands out compared to the zero interior gradient regardless of its strength. Once the interior gradient is non-zero this may change the general picture. I admit I'm not sure what specific changes to expect but I'd be curious to hear the authors' thoughts on this question.

3. It could be helpful to include some comments about the role (or lack thereof) of strength of tropopause PV gradient on vertically propagating stationary waves (see Lindzen and Roe, 1997 which is a correction to the earlier Lindzen, 1994).

Lindzen and Roe, 1994: The Effect of Concentrated PV Gradients on Stationary Waves: Correction, J. Atmos. Sci., 54, 1815-1818.

Lindzen, 1994: The Effect of Concentrated PV Gradients on Stationary Waves, J. Atmos. Sci., 51, 3455-3466.

Specific comments:

line 91: I think most readers would prefer if you copy the expression for S here (and for PV would be helpful, too)

line 100, Eq. 4: please discuss how the proportionality is to be applied (i.e., do you need to introduce a proportionality factor?)

Fig. 1, caption: I think it would help to spell out the meaning of the parameters and briefly describe the different experiments (similar comment applies to other Fig. captions)

line 198: matter of taste, but I think readability would be improved if you formulate an extra sentence for the wavelength results (i.e., avoid the short-form expressions with square brackets; similar comment applies to other places) line 260: notation for derivatives is changed here, I suggest using consistent notation

lines 457ff: I agree that it's important to end the paper with this caveat. Another caveat is that this paper only considers the growth phase of baroclinic instability and that the results reported here may not carry over to the mature and decay phases, even in idealized settings. I encourage the authors to include a related comment (and perhaps emphasize at a few places throughout the paper that they only consider this one stage of the life cycle).