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Reply on RC2

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Author 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-AC2, 2021

Response to Reviewer 2 for wcd-2021-13

We thank the reviewer for reading through our manuscript and appreciate the concise and constructive review. Below we respond to each comment and mark the response with bold font.

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.

We are happy to see that the reviewer appreciates the results and the idealised nature of the study.

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 state 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".

We agree with the reviewer that it is not fair to directly compare modifications in the PV structure around the tropopause with the introduction of new PV structures in the mid troposphere from diabatic processes. For this reason, we investigate the sensitivity to latent heating intensity by using a moderate heating intensity (middle row in Fig. 12) as a reference and compare it to cases with weaker or stronger heating intensity (upper and lower rows in Fig. 12, respectively). This way we make sure that the modifications in latent heating intensity are reasonable compared to the modifications in near-tropopause structure. The justification for the realism of the modifications in near-tropopause structures is stated in lines 147ff, whereas a similar justification for the +/- 25% modifications in latent heating intensity is indicated in lines 393-394. The uncertainty of comparable modifications in heating intensity is further elaborated by reducing the modifications from a 25% change in intensity to a 5% change in lines 398-400.

Before we compare the modifications in heating intensity with modifications in tropopause structure, we also discuss how the sensitivity of baroclinic growth to tropopause structure changes when we go from no heating to moderate heating. This part is essential for understanding why the role of the tropopause changes slightly when heating is included. Nevertheless, based on the reviewer's comment, it might not be clear enough in the original manuscript that it is not this *introduction* of heating - but rather the *modification* of heating of moderate intensity - that is used as a comparison to the sensitivity to tropopause structure. We will clarify this in Section 4.3 of the manuscript.

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.

We are happy to discuss this point of non-zero interior PV gradients. In the current model version, interior PV gradients exist when there are vertical changes in wind shear and stratification, which is mainly near the tropopause. Thus, the smoothing of the tropopause already introduces nonzero PV gradients in the upper troposphere and lower stratosphere. In addition, several previous studies (see e.g., Vallis, 2006) have added the beta effect in Eady-like models and found that the resulting interior PV gradients typically weaken the growth rate and make the structure more surface-based, because any vertical level in the interior needs to interact with the surface. With the enhanced role of the surface, we anticipate that tropopause sharpness and its associated PV gradients may become even less important than without these interior PV gradients.

Vallis, G. K., 2006. Atmospheric and oceanic fluid dynamics, p. 274-277. Cambridge University Press.

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).

We agree that this study by Lindzen and Roe addresses aspects that are of

potential interest for the broader context of our paper. However, as their study focuses on the effect on stationary waves, where modifications in the PV structure change the PV gradient in the entire troposphere, we argue that it is not directly relevant for our work. To keep the context of our study concise and clear, we therefore decided beforehand that we won't refer to this study. We hope that this decision sounds reasonable.

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)

This is a fair point that we will implement in the revised version of the manuscript. We will also make sure to point to the definition of QG PV, which is the expression inside the square brackets of Eq. (3).

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

To be clearer about the proportionality between the left and right hand side of the equation, we decided to expand this equation in the revised version of the manuscript.

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)

We thank the reviewer for pointing this out. The experiments are mentioned in lines 140-146 in the text, but we agree that they should also be briefly mentioned in the caption. We will fix this in the revised version. We will also extend the captions of Fig. 2 and Fig. 8, but decided to not repeat the description of all the sensitivity experiments in the figure captions and instead refer to the definitions in the text.

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)

This is indeed a matter of taste of either a dense formulation or a slightly repetitive formulation. We decided to implement the suggestion from the reviewer in the revised manuscript.

line 260: notation for derivatives is changed here, I suggest using consistent notation

We are glad the reviewer spotted this. We will make sure the notation is consistent throughout the manuscript.

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).

We agree that our results are probably not very applicable to later stages of

development which are typically highly nonlinear. In the original manuscript, we mentioned the focus on the incipient stage of development three places (lines 82, 181, 356). Nevertheless, we agree that we could be clearer about these limitations and decided to also mention it in the last paragraph of the conclusions.