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Reply on RC1

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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-AC1, 2021

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.

This is an interesting paper containing usefuly insights into the relative role of shear and sharpness at the tropopause. It uses an idealised framework for the investigation, which as the authors acknowledge would require further investigation. This should not be seen as a criticism however as the idealised setting provides the ideal setting for testing ideas and forming hypetheses to be tested in a more comprehensive setting as long as limitations are clearly articulated.

We are very happy to see that the reviewer acknowledges the value of the idealised framework.

I feel the following points need to be addressed.

Comments:

Ln 10: Last line of the abstract. This is slightly too strongly worded. Suggest including the word "may": "These findings may indicate that tropopause sharpness is less important for baroclinic development than previously anticipated and that latent heating and the structure in the lower stratosphere may play a more crucial role, with latent heating being the dominant factor."

This is a fair point that we will address in the revised manuscript.

Ln 185: Is it true that that temperature cannot be defined, or is it just the definition is arbitrary (e.g. like zero point of heaviside function). Can it not be defined as the limit as a smooth tropopause tends to a discontinuity or a matching condition for equations above and below the discontinuity. This is a minor point.

We thank the reviewer for raising this point. We agree that temperature can be arbitrarily defined at the tropopause level. In the model in use, the model output includes a streamfunction with a breaking point at the tropopause, such that the vertical derivative of it, i.e., temperature, is a priori discontinuous and undefined at the tropopause. In the postprocess, we have chosen to define tropopause

temperature as the average of the temperature just above and just below the tropopause, which is probably the best arbitrary definition. Such a definition should be consistent with, or at least similar to, the temperature in the limit where a smooth tropopause becomes a step function. We will modify the statement in line 185 in the revised manuscript to make this clear.

Ln 190: "However, that we obtained qualitatively similar solutions for all smoothing ranges, including the sharp experiment, indicates the suitability of QG framework to explore the sensitivity to the sharpness of the tropopause." The rationale here is not clear to me. How does consistency within the QG framework imply consistency in a more comprehensive setting? This needs to be explained more clearly or perhaps an acknowledgement that this is a limitation of the work included.

We agree with the reviewer that this is a limitation that needs to be acknowledged. Despite the caveat of self-consistency, we believe that the robustness of the results across solutions for a wide range of a smoothing and different dominance of the nonlinear vertical advection term supports the QG framework. We will, however, make sure to clarify these limitations in the revised manuscript.

Ln 210: How is the non-zero vertical velocity and consequent advection across a discontinuous tropopause justified? Surely this would lead to raising, sinking of the tropopause level. In the Eady model this is avoided by enforcing zero omega at the rigid lid. In the idealised setting discontinuous heating profiles are usually assumed to represent a change in state of the moisture - e.g. the lifting condensation level. What is the rationale for maintainence of the sharp tropopause in the present work? Simply small amplitude perturbations?

We appreciate the comment about providing a rationale for nonzero vertical motion at the tropopause. Our arguments that the shift of the tropopause level due to vertical motion is a minor effect that can be neglected in this idealised framework are twofold. Firstly, given our focus on the incipient stage of development, a feedback on the tropopause by the perturbations is small. Secondly, vertical motion is significantly reduced at the tropopause compared to the mid-troposphere.