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Reply on RC1

Markus Geldenhuys et al.

Author comment on "Orographically induced spontaneous imbalance within the jet causing a large-scale gravity wave event" by Markus Geldenhuys et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-1289-AC1, 2021

Changes to manuscript unrelated to the reviewers' comments
A minor addition have been made in Section 4.3 (CTL-run vs. T21-run: What causes the difference?). A new insight into Uccelini and Koch (1987) Eq. 9 has sparked the change. This change do not change the content nor the conclusions of the manuscript, however, the article would be more complete with this there-in.

Response to Anonymous Referee 1
We would like to thank the anonymous referee for reviewing our manuscript. The comments add value and overall improved the scientific value of the work.

Response to Specific comments
l. 43: In stating that 'most' GCMs do not resolve gravity waves properly, the authors seem to indicate that some GCMs do resolve them well. If I understand the situation correctly, those GCMs are too expensive to be applicable for climate simulations, right?

Yes, that is correct. I have added the following sentence in the manuscript to reflect this: "The few GCMs that do resolve a large spectrum of GWs are computationally too expensive for climate and chemistry runs."

*Section 2.4: What speaks against dividing the cross-stream ageostrophic wind speed by the total horizontal wind speed and using the resulting Lagrangian Rossby number as a measure of deviation from geostrophic equilibrium? This would look more intuitive to me, while the cross-stream ageostrophic wind speed only indicates imbalance when it is comparable to the total horizontal wind speed. Moreover, many experts would not accept the identification of spontaneous imbalance with geostrophic adjustment (e.g. Plougonven & Zhang 2014). The first is a true emission process, while the second is an initial-value problem. I would encourage the authors to keep these things better apart from each
other.*

This is a good comment. Some of the following reasoning has been included in the manuscript.
In Zülicke and Peters (2006) it is argued that the cross-stream ageostrophic wind velocity can equally serve to diagnose an unbalanced flow field - an idea which originates from

quasi-geostrophic theory tracing back to Koch and Dorian (1988). Later, Mirzaei et al. (2014) use a threshold for the cross-stream ageostrophic wind speed. They go further in saying that this is comparable to using an ageostrophic Rossby number for selection of flow components which are faster than the Coriolis parameter (f). Further, their approach has less 'noise' than simply using the ageostrophic Rossby number. In our study, a similar more noisy dataset is obtained when using the cross-stream Lagrangian Rossby number.

The following has not included in the manuscript:
Using ERA5 data to illustrate the effect in our study, Figures 1 and 2 (attached here) show similar results for the cross-stream ageostrophic wind and the cross-stream Lagrangian Rossby number. The cross-stream Lagrangian Rossby number covers a larger region and is noisier than the cross-stream ageostrophic wind, but both show unbalanced flow within the same region.

Figure 1: Cross-stream ageostrophic wind calculated at 350hPa from ERA5 data. The top left panel starts on the 9th of March 2016 at 18:00 UTC and continues in a 6hrly timestep to 11th at 00:00 UTC. Data was only plot where the total wind speed was greater than 20 ms^{-1} and for cross-stream ageostrophic wind values less than 5 ms^{-1}.

Figure 2: Same as for Fig. 1 for the variable Cross-stream Lagrangian Rossby number.

Ls. 227-228 and table 1: Where in table 1 do I see a vertical wavelength? Or is this lambda_y? But then the caption would be incorrect.

Thank you this slipped through! This has been corrected to lambda_z.

l. 269: How does a decrease in stability lead to a decrease in the vertical wavenumber? Is it not the other way round? At constant intrinsic phase velocity one would have N/m constant, with m the vertical wavenumber. This is for the mid-frequency range, but I would assume this is not changed substantially if the intrinsic frequency is close to f?

A wave duct is formed when a more stable layer is sandwiched between two less stable layers. Wind speed and stability is known to be responsible for creating a wave duct. Although the wave duct is not strong enough to reflect the wave downwards (as discussed in Section 3.1 in the manuscript), a little energy will still be lost. This is what we wanted to touch on, but we see we did not bring this message across.
However, you make a valid point that a decrease in stability leads to an increase in vertical wavelength. (I assume you meant vertical wavelength in your comment and not vertical wavenumber as is written. A decrease in stability will cause a decrease in vertical wavenumber.) Hence, I decided to avoid confusion and update the text accordingly.

l. 10: the the is one the too much.
This has been resolved.

References

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Please also note the supplement to this comment:
https://acp.copernicus.org/preprints/acp-2020-1289/acp-2020-1289-AC1-supplement.zip