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Comment on acp-2020-1289

Anonymous Referee #1

Referee 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-RC1, 2021

General comments:

This manuscript describes an interesting analysis of a gravity-wave signal, measured over Greenland by the GLORIA instrument from the HALO aircraft. Ray tracing and high-resolution simulations using IFS are used to interpret the observed signal. By backward ray tracing it is found that the gravity wave originates in the jet. An attempt at diagnosing imbalance via the strength of the cross-stream ageostrophic wind is made, yielding indications that the diagnosed wave has passed (and may have originated from) unbalanced locations tending to emit gravity waves. The most important result is obtained from IFS simulations. These show that the Greenland orography, though not emitting the wave itself, yet plays a crucial role in configuring the large scale flow so that the necessary degree of imbalance is reached. Hence the source process is a kind of stimulated imbalance due to the orographic impact.

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?

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.

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.

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?

Technical corrections:

l. 10: the the is one the too much.