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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Gravity Waves excited during a Minor Sudden Stratospheric Warming" *by* Andreas Dörnbrack et al.

Anonymous Referee #3

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This is an interesting manuscript providing several indications for an observation of the excitation of internal gravity waves by the spontaneous adjustment of a sub-vortex at the inner edge of the Arctic polar vortex during a minor sudden stratospheric warming. Unlike most scenarios considered to date, where the spontaneous excitation occurs in the troposphere, e.g. in jet-exit regions, here this process seems to be at work in the stratosphere. The authors make their case by an impressive combination of analyses, both of observational radiosonde data and of IFS simulation data from ECMWF. Several strong indicators are presented, e.g. upward/downward energy fluxes above/below the presumed emission region, corresponding downward/upward phase propagation, Stokes analyses applicable to nearly monochromatic wave fields, a configuration of the balanced flow (diagnosed by a modal analysis) conducive to spontaneous emission in a

way similar to typical tropospheric situations, and wavepacket reconstructions verifying the assumed vertical propagation directions. I therefore recommend acceptance of the manuscript, provided my comments below are taken into account.

Main Review Points

1. As tempting as the conclusion on spontaneous emission seems to be, I am not (yet) convinced completely:

a. The dynamical situation is discussed using to a large part Figs. 4 and 5, that show a complex situation with several wave packets due to different origins. The focus is on a comparatively weak signal over Northern Scandinavia that is co-located with a sub-vortex formed at the edge of the polar vortex. As I understand the authors, they base their assumption of spontaneous adjustment on the jet deceleration observed there. This might be correct, but appears somewhat speculative nonetheless. It might be impossible to provide a real proof, but the authors could collect more evidence for the balanced flow getting out of balance. A standard indicator is the local Rossby number, e.g. determined from the ratio between relative and planetary vorticity, that should be larger in regions of spontaneous emission.

b. Based on low potential-energy values, the authors argue in the conclusions against the reflection of mountain waves. This sounds reasonable, but can one exclude that a low-frequency gravity wave, approaching the jet obliquely from below, is reflected by the latter? How can one make sure that such a reflection process is not taking place at some horizontal distance, and that one locally obtains a situation that looks like emission but really is refraction by the jet? I would do not have a problem if the basis of evidence remained as it is now, but then I would recommend formulating abstract and conclusions even more openly, and maybe also changing the title to 'Indications for . . .', e.g.

2. The interesting modal analysis, used for separating balanced flow from gravity waves, could be exploited even more:

a. If (!) the authors were interested I would find it really useful to see the wavelet analysis of the IFS data redone, most importantly Fig. 9, for the IGW part only. As in many other publications the authors directly identify the deviations from some mean with gravity waves. This assumes (!) that balanced motions do not contribute significantly to the small scales, but here would be a chance to demonstrate this for the given case.

b. Also, in an imbalance analysis using local Rossby numbers, how much do the mesoscale balanced motions contribute to the result?

Minor review points

1. p. 4, l. 28: “semi-implicit model” does not sound correct. It is just the time step that is semi-implicit.

2. p. 5, section “Scale-dependent . . .”: I sympathize with the linear modal analysis, yet it is linear and a short comment might be in place, why no higher-order, nonlinear, balance concept is used, as are discussed by Plougonven and Zhang (2014), e.g.

3. p. 6, ls. 14 - 19: Kaifler et al (2015, 2017) do not use quite the same relative spectrograms. They subtract the average instead of the sum. Would be better if this were described correctly.

4. ps. 8 – 10, section “Gravity wave analysis”: Here and at many other places local fluctuations are identified with gravity waves, and additional analysis is done to demonstrate the consistency of this assumption. Often this is unavoidable, but here modal analysis of IFS data could be used to support this hypothesis even more.

5. p. 9, ls. 8: Rather “dominated by” than “have”? Some of the smaller-scale fluctuations, that IFS does not simulate, might have larger frequencies.

6. p. 11, ls. 7 - 8: I do not quite agree that modal decomposition and horizontal divergence are independent diagnostics. By its polarization relations, being geostrophic, extratropical balanced flow, as diagnosed from the modal analysis, is non-divergent.

Moreover, both analyses might incorrectly attribute divergent flow to gravity waves, while it might as well belong in parts to the balanced flow, as e.g. can be diagnosed from the omega equation and other higher-order balance concepts. I agree that it is meaningful to investigate horizontal divergence, but one should also admit possible issues . . .

7. ps. 11 – 12, section “Gravity waves”: This is the section that I find somewhat speculative, see my major comment 1.

8. ps. 13 – 14, section “Wavelet Analysis . . .”, esp. Fig. 9: Here the assumption is used that vertical gravity-wave phase velocities oppose the corresponding group velocities. Fortunately, in the conclusions the authors mention the risk of misinterpretations due to the Doppler term, and invoke energy flux as well. However, in the present case where all information is available in the IFS data, would it not be better to avoid this risky analysis, and rather show group velocities directly?

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