Author's response

October 14, 2021

To the editor

We thank the reviewers again for their constructive comments. In this revision, we addressed the comments of Reviewer 1 asking for clarity in a few statements by reworking our text in the indicated places. We still don't agree with Reviewer 3's comment on correction of our Appendix A. However, to convey our statement more clearly we have revised Fig. A1 and the text in a few places.

Reply to Reviewer 1

(304) "Various sources excite gravity waves of scales typical...": This strikes me as opaque. The point is that the wavenumber spectrum generated by a source depends on the spatial scale of the source. How about something like "Sources of gravity waves generate a wavenumber spectrum that depends on the physical dimensions of the source"?

We have replaced the sentence by your recommendation.

(307) "the spectral distribution": Do you mean the wavenumber spectrum? Spectral distribution could also refer to the frequency spectrum.

Yes, we do. We have specified that in the manuscript accordingly.

(308) "descried [sic] by the spectral distribution gained from the monochromatic waves in this region": I believe you meant "described" instead of "descried". That aside, I do not understand what you mean by "the distribution gained from the monochromatic waves". What monochromatic waves are you referring to?
Note that all of this confusion appears to have arisen following my original comment that any real-world topographic source will excite a spectrum in wavenumber: For the very simple example of a Gaussian source, the wavenumber spectrum is a Gaussian red spectrum, not a spectrum "centered at the main carrying frequency" [sic—I presume you meant wavenumber here]. That is, the Gaussian width, L, of an obstacle in physical space, h' = exp[−x2/ (2 L2)], is inversely related to the Gaussian width of its spectrum in wavenumber space, exp[−k2 /(2 k2]), with k = L-1. Now, I presume all that you mean is that the physical appearance of the wavepacket excited by orography is dominated by wavelength k -1 L, which is fine. But it would be useful to state things more clearly.

We have reformulated the respective paragraph. The intention here was to state the fact that a few wavenumbers in local fits are usually enough to describe the wave composition. These few waves then allow meaningful propagation studies of localised wave packets.

(469) "broad an apparent contradiction forward": I think you mean "brought forward an apparent contradiction", or more simply "highlighted an apparent contradiction". We have adapted the text according to your recommendations. This expression is indeed much better!

(477) "require a modeling study upwards from relevant sources": It might be clearer to write "require a modeling study of upward propagation from relevant sources".

Thank you for the suggestion. We have adapted the text accordingly.

(488) “wind and intrinsic phase speed are of the same magnitude but opposite to each other $((U\hat{c}) = c_{gb} \approx 0)$”: This does not make sense because $U - \hat{c}$ is the intrinsic phase speed. I think the source of the confusion is in the use of the word “intrinsic”. The common use of the term refers to the value relative to the background flow. See for example: `https://glossary.ametsoc.org/wiki/Intrinsic_wave_frequency` **In any event, for mountain waves the phase speed relative to the ground is (using your symbol) $\hat{c}$, and $\hat{c}$ 0 because the waves are generated by wind blowing over fixed topography (a nonsteady wind can still generate a spectrum in frequency/phase velocity, but that is another matter). Insofar as $\hat{c}$ 0, the waves do not propagate with respect to the ground, but do so with respect to the background flow, with intrinsic phase velocity U and intrinsic frequency kU. The slope of the wave ray with respect to the source (for the limiting case of hydrostatic, plane-parallel waves, (k/m)2 ¡¡ 1) is Dx/Dz = cgx/cgz −m / k; thus, for positive U the ray tilts toward negative x with increasing altitude. Nothing stated here invalidates any of your conclusions, but it is important to get these explanations straight and set them down clearly.**

Thank you for the detailed explanation. We agree with your assessment that our description might have been confusing and tried to clarify it by simplification. The main point we wanted to convey here is the fact, that in the special case of a pure“ mountain wave the wave intrinsic phase speed and the background wind form a balance where on compensates the other.

(488) “intrinsic group velocity”: Note that this is redundant. Group velocity is defined in terms of intrinsic frequency, k (U − c).

This part was removed in the course of the previous change.

(561) “that is only” =¿ that is, only

We have added the colon.

(563) “discerns”: I think you mean “distinguishes”.

We agree and have exchanged the wording.

(643) “This would mark a paradigm shift ... from waves that are horizontally refracted ... to waves that feature southward orientation already at source altitude”: A minor comment here is that this statement strikes me as overly dramatic. The fact that there may be sources that produce wavetrains with group velocity oriented in a non-zonal direction is hardly a paradigm shift. In some regions, such waves might dominate, whereas in other situations the refraction mechanism might be important. Note, by the way, that in studies such as Sato (2009) refraction into the core of the SH polar night jet occurs over large vertical distances, and the mechanism was proposed to explain forcing at 0.1-1 hPa, i.e., in the upper stratosphere and lower mesosphere.

We have toned the statement down a little. The final sentence reads now: Our case study therefore suggests to shift the focus of investigation from waves that are horizontally refracted by strong wind gradients into the polar night jet to waves that feature southward orientation already at source altitude, which could be more important also in general.

Reply to Reviewer 3

The scheme as presented *in the appendix* **needs correction by substitution of ”hor. phase speed” by ”intrinsic group velocity”. See Fig. 6 of Sato et al. (2012) which is also constructed of group velocities. ...**

It is correct that the schematic drawing of Sato et al. 2012 refers to group velocity. Here we want to make the specific point that in mid-frequency approximation a mountain wave propagates along it’s phase fronts. This requires the, in the community well known, equality of horizontal phase speed along the horizontal wave vector and horizontal group velocity. The use of a horizontal phase speed in the direction of the horizontal wave vector is one of the basic definitions used in the GW community. The whole fundamental discussion of critical level filtering, which is for instance basis of all GW parametrization schemes, founds on it. We have discussed this in length in our previous reply with several high ranking examples from the literature given. It is, however, true that the equality of horizontal phase speed and horizontal group speed is given only for mid frequency GWs. We will make that more transparent by adding a comment to the figure. The text to the figure now reads:

Schematic view of the horizontal propagation of a mountain wave. When there is an angle between wave vector and wind, the resulting ground-based group velocity is along the phase line. This relation holds strictly for mid-frequency mountain waves, for which horizontal group velocity and horizontal phase speed are equal, and is an approximation for high-frequency mountain waves.