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Atmospheric Chemistry and Physics Discussions

Interactive comment on "On the origin of the mesospheric quasi-stationary planetary waves in the unusual Arctic winter 2015/16" *by* Vivien Matthias and Manfred Ern

Anonymous Referee #3

Received and published: 29 January 2018

This is an interesting paper reporting an investigation of large amplitude stationary planetary waves observed in the mesosphere during the winter of 2015/16. This was a time when there was an unusually strong polar night jet. The origins of the stationary planetary waves of wavenumber 1 and 2 (i.e., SPW1 and SPW2) are investigated in detail in the context of the excitation and propagation of the waves. It is suggested that the high-latitude SPW2 is forced by longitudinally-variable gravity-wave drag, but that at lower latitudes, this forcing is increasingly replaced by barotropic/baroclinic instabilities, which eventually dominate by subtropical latitudes. The authors make use of absolute gravity-wave momentum fluxes derived from SABER observations. The paper is a nice example of the powerful insight into gravity-wave forcing of the atmosphere that can be

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provided by such measurements.

The paper is very well written, clearly argued and presents scientific results of considerable interest and of the highest quality. Overall, this is an excellent study which fully deserves publication in ACP.

I have two particular requests for changes before the paper can be accepted for publication.

My first request is that the authors expand on their explanations of the SABER measurements. In particular, there should be a more expansive description of the technique and its limitations. At present the measurements are simply described as being of "absolute GW momentum fluxes" and the reader is provided with references for more substantive explanation of the technique.

However, the SABER measurements are a central part of the paper and there should be i) a paragraph of explanation describing the technique at the point where it is introduced and ii) some discussion of the limitations of the technique. In respect of the latter, I believe that these GW momentum flux measurements actually yield a lower bound rather than a fully-constrained value, since the estimates of horizontal wavelength depend on the angle between the satellite's orbit and the phase fronts of particular gravity waves.

The arguments on p14 in paragraph 2 about the longitude-altitude cross section of wavenumber 1 filtered winds and the non-uniform GW drag at 50N with a wavenumber 1 structure refers to figures "not shown" – these figures would confirm the arguments being made by the authors so they should be included.

The arguments about the wavenumber 2 component of gravity-wave drag associated with Fig 7 would be strengthened by some explanation of the total drag and its other component wavenumbers. There does seem to be a wavenumber 2 component as shown, but how big is it compared to the zonal-mean value and the other wavenum-

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bers?

My second request is that some of the figure be made larger. As presented, some of the contours in Figs 2c, 3c, 4 and 7 are very faint, hard to read and/or close together. I think that just making the figures larger would solve this problem

MINOR POINTS

P1, l15, suggest "...show that all three mechanisms..."

Figure 1 caption, suggest "...from MLS temperature..."

P3, l21, the final sentence "The vertical propagation of... (Lin, 1982)" would make much more sense if put at the start of that paragraph.

P4, l17, should be "...winds are needed".

P4, l20, should this be aˆ(-1) $\partial/(\partial$âĹĚ) ?

P5, l5, suggest "...which results in a westerly wind..."

P5, l9, suggest "...and following it up into..."

P5, l14, suggest "...TIMED satellite and measures temperatures..."

P5, l16, suggest "...geometries about every 60 days. For the period of..."

P8, l2, suggest "...Period I, as is the areas of..."

P11, Fig 5, what causes the missing data at days -2 to 0?

P14, l3, suggest "...not able to investigate whether wave (d)..."

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2017-1051, 2017.