Interactive comment on amt-2020-14

General comments:

This work reports on gravity wave activity derived from hydroxyl airglow temperatures from a network of (mostly) European stations.

Seasonal variations in gravity wave activity are found to be dependent on wave period, from no seasonal variation (periods < 60min), to a semi-annual variation (periods 60-200min) and then an annual variation (periods > 200 min). This appears to be consistent across all stations and is the primary result of the work.

The paper is based on the analysis of contiguous series of OH temperature observations at these stations using a wavelet transform method, and compares the results to previous work using the same data. Although the results from each station are presented in Figs 2, 4 and 5 I think more can be discussed to compare and contrast the GW regimes between these sites.

The discussion and commentary around secondary wave generation in and above the stratosphere should be expanded. This is the key outcome of the work as one would expect the annual cycle of the stratospheric jet would lead to an annual GW activity cycle. Why should GW's in the period range 60-240min be produced above the stratospheric wind fields to produce the summer maximum in the semi-annual pattern of GW activity, but not periods below or above that range? Could there be other sources of summertime wave activity or possibly interaction between source variations, GW phase speed and the background wind? Key papers in this area by Becker and Vadas are lacking and should be referenced. Eg.

Becker, E., M. Grygalashvyly & G. R. Sonnemann, 2020: Gravity wave mixing effects on the OH*-layer. Adv. Space Res. 65, 1: 175-188, doi:10.1016/j.asr.2019.09.043.

Seems particularly relevant and should be referenced and also:

Becker, E. & S. L. Vadas, 2018: Secondary Gravity Waves in the Winter Mesosphere: Results From a High-Resolution Global Circulation Model. J. Geophys. Res. Atmos. 123, 5: 2605-2627, doi:10.1002/2017jd027460.

Vadas, S. L. & E. Becker, 2018: Numerical Modeling of the Excitation, Propagation, and Dissipation of Primary and Secondary Gravity Waves during Wintertime at McMurdo Station in the Antarctic. J. Geophys. Res. Atmos. 123: 9326-9369, doi:10.1029/2017jd027974.

Vadas, S. L. & E. Becker, 2019: Numerical Modeling of the Generation of Tertiary Gravity Waves in the Mesosphere and Thermosphere During Strong Mountain Wave Events Over the Southern Andes. J. Geophys. Res. Space Physics 124: 7687-7718, doi:10.1029/2019JA026694.

There are some references and comparisons to quite a few studies of radar derived seasonal GW variance (eg Hibbins (2007), Beldon and Mitchell (2009) but there are many other on this topic which are relevant and could be included. For example

Tsuda, T., Murayama, Y., Nakamura, T., Vincent, R.A., Manson, A.H., Meek, C.E., Wilson, R.L., 1994. Variations of the gravity wave characteristics with height, season and latitude revealed by comparative observations. Journal of Atmospheric and Terrestrial Physics 56 (5), 555–568

Senft, D.C., Gardner, C.S., 1991. Seasonal variability of gravity wave activity and spectra in the mesopause region at Urbana. Journal of Geophysical Research 96, 17229–17264

Manson, A.H., Meek, C.E., 1993. Characteristics of gravity waves (10 min–6 h) at Saskatoon (521N, 1071W): observations by the phase coherent medium frequency radar. Journal of Geophysical Research 98 (D11), 20,357–20,367.

Thorsen, D., Franke, S.J., 1998. Climatology of mesospheric gravity wave activity over Urbana, Illinois (401N, 881W). Journal of Geophysical Research-Atmospheres 103 (D4), 3767–3780.

and also imager data for example eg.

Nakamura, T., Higashikawa, A., Matsushita, Y., 1999. Seasonal variations of gravity wave structures in OH airglow with a CCD imager at Shigaraki. Earth, Planets and Space 51, 897–906

When comparing these observations it should be considered that due to the OH layer width observations based on OH will only see waves with vertical wavelengths greater than about 14 km whereas radars will not have that selection and may see a different population of GWs. In particular, you might be seeing low GW activity in the short period waves (< 60 mins) because they have short vertical wavelengths and the perturbation is averaged out through the OH layer.

Consider what modelling studies have been done in this area in regard to GW interaction with airglow layers and to the seasonal variability of different period bands? (GSWM has been mentioned but not its expectations for seasonal variability). The Vadas and Becker work (mentioned above) have been active in numerical modelling and are particularly relevant to the current study, and also England et al., 2006 and references therein for the CMAT model.

England, S.L., Dobbin, A., Harris, M.J., Arnold, N.F., Aylward, A.D., 2006. A study into the effects of gravity wave activity on the diurnal tide and airglow emissions in the equatorial mesosphere and lower thermosphere using the Coupled Middle Atmosphere and Thermosphere (CMAT) general circulation model. Journal of Atmospheric and Solar-Terrestrial Physics 68, 293–308

After due consideration is given to these aspects I would consider the manuscript appropriate to publish.

Further specific comments, corrections and suggestions:

Page 1. Line 28 “temporal course” -> evolution.

Page 2. Line 10. “... Region *for* gravity…”

Page 2. Line 15. The variation of wind or temperatures caused by gravity waves implies the same meaning for ‘gravity wave activity’. The presence of wave activity can be detected by either parameter as both are perturbed in the same matter by the wave. The *properties* (period/speeds/wavelength) of the waves may differ subject to the sensitivity of the measuring technique to different parts of the GW spectrum.

Page 4. Line 5. The fact that measurements are only possible during the night time is not “due to solar radiation”. It is due to a limitation of this technique being unable to detect the OH* emission above the solar background, and the different photochemistry involved during the daytime. This sentence needs revising.

Page 4. Heading “Data Bases”

Page 4. Line 7. Omit ‘its’

Page 4. Line 19. Omit “Apart from that”

Page 4. Line 21 Omit “the”

Page 4. Line 28. How is the temperature uncertainty determined ?.

Page 4. Line 30. I don’t think “succeeding” is the right word. Do you mean “successive” ?

Page 5. Line 4. “For *the* high-latitude stations (NEU,ALR) no observations can be made during the polar summer”

Page 5 Line 5. “Also *the* Alpine stations (SBO,UFS) show *minimal* observations during summer, principally due to bad weather”

Page 6. Line 5. “wavelet analysis *method*”

Page 6. Line 6. “then delivers” -> provides or yields

Page 6. Line 16. “In the further course of “ -> “Later in”

Page 6. Line 19. “short-period” omit “ic”.

Page 6. Line 27+. From your synthetic data analysis you should quantify the ‘slightly higher’ peak intensities of shorter period waves relative to longer period waves. I.e. for a synthetic wave field of a sequence of equal amplitude waves spanning the GW spectrum region of interest what are the relative wavelet intensities for each period passing through your analysis?. That would go some way toward linking “absolute values of wavelet intensity to actual temperature amplitudes” and provide a scale for Figs 2-5 rather than just arbitrary units.

Page 7. Line 3. The term “nocturnal mean of the significant wavelet intensity” is used several times in the manuscript (eg Page 7. Line 12. and Page 8. Line 5.) but to me implies a single value (the mean of wavelet intensities that have passed the 99% significance test).

Page 7. Figure 2.
60 and 200 min period markers too small to read.
Isn’t the colorbar a ‘relative wave amplitude’ rather than arbitrary units ?. For example do black patches signify waves that are approximately twice the amplitude as orange?.
It appears the shading has been interpolated across months which is somewhat artificial. I would prefer to see the monthly values plotted in blocks so that month to month differences can be assessed.
Omit “Long term courses of” from the caption.

Page 7. Line 4. “average behaviour”. What average ?. Do you mean the period distribution and/or seasonal variability is quite similar?.

Page 7. Line 5-6. It is very hard to distinguish anything below 60 mins, let alone 25 mins on Fig 2. Would a log-period plot show what you describe better ?.
It should be considered here what the effect of the vertical wavelength has on the observation of GW using the OH airglow layer. Short vertical wavelengths will not be seen by these observations. Are these associated with short period GW’s?. Comment and discuss.

Page 7. Line 9. Dashed line at 200min for ABA is missing.

Page 7. Line 11 “using OPN data”

Figure 3. This figure does not entirely support the statements that are made in the previous paragraph. I see a strong semi-annual variation starting in the blue range (~10 mins) right through to the orange range (~350 mins), then the annual cycle above that. Indicate on the figure the 25 min, 60 min and 200 min boundaries for the change of seasonal character referred to in the text. The first sentence of the figure caption should be corrected as for Page7. Line 3.

Page 7. Line 12+. This paragraph and the selection of peaks in the standard deviations of wavelet intensity is not convincing and I question whether it is “interesting”. The local peak at 45min for example appears to only occur in OHP and NEU really, and the 105 min peak only in UFS, SBO and ABA. Why are the local peaks identified of interest? Why even identify peaks in the SD of wavelet intensity which would indicate high variability and inconsistency in those periods rather than troughs which would indicate a consistent periodicity?

Page 8. Line 6. And Fig 5. Why 60-480 min and not 240-480 min range as the third interval ?. You have already covered the 60-240 min periods and you have previously identified ~200 min as the transition from semiannual to annual variation. The 60-240 and 60-480min periods are very similar as seen in fig 5.

Page 8. Line 10 course -> variation

Page 8. Line 10+ .. I would not say the annual variation is “dominant” in the stations identified, but rather that both annual and semi-annual variations are apparent (as I would expect looking at Fig 3). Perhaps if you provided the 240-480 min period range the annual cycle would be more apparent?

Page 8. Line 16 “undisturbed” -> “contiguous”

Page 8. Line 16+. I am not sure of the point of this discussion. You are using data from the same stations as Wust et al. (2016, 2017), with some further restrictions required for contiguous time series, and processing data for the same periodicities. It is no surprise that your findings agree well. If the same results and conclusions can be reached using the previous method, what are the significant advantages of the wavelet analysis method?

Page 8. Line 18. What fraction of data is unavailable for this analysis that can be analysed using the method in Wust et al. and does this highlight the limitation of this method that requires contiguous data.

Page 8. Line 24 “periodic” -> “period”

Page 8. Line 19. “us” -> “this work”. Data basis -> “data base” and line 20.

Page 9. Line 12. To “a” westward

Page 9 Line 26 Minimum -> “The minima in”

Page 9 Line 27. Episodes -> times

Page 10 Paragraph 1 – as for comment Page 7. Line 12+. I did not find the "most prominent" peaks in the SD plot in Fig 4 to be particularly convincing and is it of significant interest to identify GW periods that are infrequently and inconsistently observed compared to identifying GW periods that are consistently observed (low SD) and contribute to the structure and state of the UMLT.

Page 10 Paragraph 2 – This paragraph on the comparisons between stations would be better suited following on from paragraph 1 in the discussion section (Page 8 Line 16) and expanded to compare and contrast these stations as this is the observational strength of the work. Apart from adding (or modifying) to show the 240-480 min period range, the similarities and uniqueness of each site with respect to geophysical source regions should be discussed. You attribute higher mean wavelet intensity for long-period waves to "regional peculiarities in the Caucasus" for example. The reader must assume perhaps you mean greater orographic GW forcing in this region? Why should this be the case when other sites are also located in mountainous regions? Why is there a dip in long-period GW intensity at TAV in September?. All stations appear to show a maximum in short-period (6-60 min) GW intensity in Jun-Jul, but this this does not appear to be a summer effect as the same is true for NEU. A regional map of the European sites and potential orographic GW source region may help in this discussion. Include the polar sites of ALR and NEU in the comparison.

Page 10. Line 10. agrees with -> supports

Page 10. Line 11. Prominently -> importantly stratospheric

Page 11. Line 3. "have been" -> were

Page 11. Line 8. The significance of being situated in mountainous regions is that these would be expected to be significant sources of orographic GW's.

Page 11. Line 9 "turns out to be" -> "is observed to be"

Page 11. Line 14. As above the 6-60min variations in Fig 5 appear to show a maximum in Jun-Jul for all stations (excluding ALR with no data in those months)

Page 11. Line 19. I do not think the distinction is made here on the basis of Figure 5.

Page 11 line 25. Your measurements are not a proxy, they are in-situ observations.