The quasi 16-day wave in mesospheric water vapor during boreal winter 2011/2012

D. Scheiben, B. Tschanz, K. Hocke, N. Kämpfer, S. Ka and J. J. Oh
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Response on the comments from Referee #1:

We thank Referee #1 for his comments on our manuscript. In the following, we answer these comments point-by-point. The Referee's comments are given in *green and italic* font, our answer in black.

General comment:

- *The present paper describes the analysis of the quasi 16 day variation in water vapour in the upper stratosphere and mesosphere during boreal winter 2011/2012 based on observations by three ground-based radiometers located at mid- and high latitudes. The results are complemented by and compared to observations by the Microwave Limb Sounder instrument aboard the Aura satellite. In general the paper is well written. Occasionally there could be some more information to guide the reader better but overall I have only few comments. Once addressed I recommend the publication of the manuscript in ACP, fitting well into the scope of the journal.*

Specific comments:

- *Lines 21 – 22: "The dynamical regime … is characterised by global-scale planetary waves…" - That seems a little bit too much of credit for planetary waves, in particular in the mesosphere where gravity waves play a decisive role for the dynamics and the overturning circulation.*
  In the revised manuscript, we reworded this sentence and mention global-scale waves among others to be characteristic for the middle atmosphere.

- *Lines 33 - 34: "… known to be a major driver of atmospheric dynamics in the winter-time middle atmosphere …" - To me, it looks like this is attributed to all planetary waves listed in line 30 and 31. I just like to add for consideration that, for example, the quasi 2-day is more pronounced during summer time (e.g. Limpasuvan and Wu, 2003). Also the quasi 5-day wave plays an important role during summer (e.g. Sonnemann et al., 2008).*
  Vertical wave propagation is attributed to all types of atmospheric waves, not only to the ones we mentioned in our manuscript. We changed the corresponding sentence slightly to make clear that this attributes to all types of waves, but that the focus of our manuscript is on the quasi 16-day wave.

- *Line 64: "… atmospheric tides (e.g. …" - Here I would definitely add a reference to the paper by your close colleague Alexander Haefele and co-workers (2009), who to my knowledge was the first to address this topic.*
  You are correct. We added a citation to Haefele et al. (2009).

- *Lines 80 - 82: Other factors that immediately come to my mind, defining the vertical range of the observations, are the band width and spectral resolution of the radiometer. Maybe it is worthwhile to mention that already here and not, as done, later on. Also the integration time has some influence. Which integration time has been used, anyway? I did not find this information.*
  Since we used three different radiometers with slightly different characteristics, we preferred to include the information on bandwidth, spectral resolution and integration time in the sections of the corresponding radiometers, and not in the general section before going into the details of each radiometer.

- *Section 3.1: Which terms are included in the regression? Only offset and sine and cosine? Or also a linear term? Maybe an equation would be a good idea? What is resolution of the time series? 12 hours? What is the measurement coverage during the time period in question? It is certainly not 100%, but how much is it?*
  For the regression model, we use an offset, a linear fit and a sinusoidal term. This information is now included in the revised manuscript. As already mentioned in the manuscript, the resolution of the time series is varying due to the varying integration time of the water vapour retrievals, which is why we could not apply a Fourier transformation. Besides the few data gaps mentioned in Section 2 of the manuscript, there are no other data gaps, hence the measurement coverage is almost 100%. Most importantly, all data gaps have durations of less than 5 days, such that their influence on the investigation of the quasi 16-day wave is minimal (e.g., through aliasing).

- *Section 3.2: According to Figure 2 now also data from November 2011 is used. That feels a bit inconsistent.*
  Figure 2 shows the band pass filtered water vapour time series from 01 November 2011 until March 31 2012. November is only shown in this Figure to show the increase of the wave activity in early winter.

- *Line 165: "Therefore only wave numbers up to ±4 are shown." - This sentence is redundant.*
  We deleted this sentence.

- *Figure 1: In addition it would be very interesting to see those results not only in absolute but also in relative terms. Adding some significance contours would be helpful to distinguish between noise and real results.*
  The increase in the relative wave amplitude with altitude only starts just at the upper measurement limit of our radiometers, i.e., above 0.02 hPa. This is the reason why the increase in relative wave amplitude cannot be observed with our radiometers, since we simply do not have measurement sensitivity above 0.02 hPa. Therefore, the figures are only shown in absolute units.

- *Figure 3: Which criterion did you use to define the onset of the SSW?*
  For the onset of the SSW, we used the definition of a SSW according to the WHO, i.e., we chose the first day where the temperature gradient on 10 hPa was positive northward of 60°N. The revised manuscript contains this information.

- *Section 4.3: How are the Aura/MLS data compiled? Are time period and resolution consistent with the ground-based data? The discrepancy between the results from water vapour and geopotential height is somewhat puzzling. I think that deserves some discussion.*
  The time period of Aura MLS is consistent with the time period of the ground-based data. At 0.05 hPa, the vertical resolution of the Aura MLS water vapour retrieval is approximately 8 km, compared to approximately 14 km of the ground-based radiometers. The vertical resolution can therefore be regarded as similar. We did a convolution of the MLS data with the averaging kernels of the ground-based radiometers and the results look very similar (for Figures 5 and 6). Waves in water vapour and geopotential height are not necessarily correlated, since waves observed in mid-latitudinal mesospheric water vapour could originate from vertical or meriodional displacement of air or (most likely) are a mixture between the two, whereas the vertical and meridional gradients of water vapour and geopotential height can be very different.

- *Section 4.4: From Figure 5 it looks like that the Aura/MLS data are binned into 20° longitude bins. That should be at least mentioned in the text somewhere. Also, there is some mismatch between the altitude resolution of the Aura/MLS and ground-based observations. In Section 2 an altitude resolution of 17 km is mentioned for the groundbased observations in the middle mesosphere, while based on the averaging kernels given on "https://mls.jpl.nasa.gov/data/ak/" the altitude resolution of Aura/MLS is in the order of 6 km - 8 km at 0.05 hPa. Since there are pronounced gradients in the water vapour profile around this pressure level, I would expect, at least, some differences in the comparisons. I would recommend one test time series where you degrade the Aura/MLS onto the vertical resolution of the ground-based observations to check what influence the resolution really has. Otherwise, I guess, using relative amplitudes helps masking any potential issues.*
  In the revised manuscript, we now describe how the MLS data are averaged into 20 degrees longitude and 2 degrees latitude bins for the results in Figs. 5 and 6. Regarding the vertical resolution: The MLS data quality documentation (http://mls.jpl.nasa.gov/data/datadocs.php) shows that the vertical resolution of the v3.3 water vapour retrievals degrades rapidly above 0.22 hPa to a vertical resolution of approximately 8 km at 0.05 hPa. At this pressure level, the vertical resolution of our ground-based radiometers is approximately 14 km, hence it can be regarded as comparable to the Aura MLS resolution. We did make a test time series with the Aura MLS data convolved with the averaging kernels of our ground-based radiometers, but the results look very similar to the results with unconvolved Aura MLS data (for Figs. 5 and 6). Relative amplitudes are used in Figs. 5 and 6 for a better comparison between all data sets due to known biases among the instruments (which are described in a validation paper of Haefele et al., 2009).

- *Figure 5: Personally, I found it confusing to not constrain the phase plot to -180° - 180°.*
  We did not constrain the phase plot to -180° - +180° but to -200° - +200°, because this way, it is easier to interpret the data points close to the "transition zone" at -180°/+180°.

- *Line 299: "… but shows two distinct regions with two different phases." - I am not sure I would come to the very same conclusion?*
  Our interpretation is such that the wave at 67°N does not travel in the region between 180°W and 45°W and in the region between 0° and 120°E, which corresponds to the regions of the highest wave activity. There is a phase difference between these two regions. This is why we state in the manuscript, that there are two distinct regions with different phases.

- *Lines 301 - 303: "The phase difference in Seoul can be explained by the fact that the 20-day wave above Seoul is practically inexistent and therefore the phase difference is insignificant." - Yet, the amplitude at 37°N/110°E is even smaller but the phase looks very reasonable. So, that is certainly not the best chain of arguments. Looking at the phase in Figure 5 and 6 the Seoul data point (in Figure 5) really seems to be the only one that sticks out completely. Somehow that gives a sneaky feeling that there may be something fishy with the data and/or analysis. Sorry, I cannot help this suspicion. If not, it seems very unfortunate, as everything else looks very consistent.*
  The problem here is that it is very difficult to adequately determine the phase of a weak wave that is superposed by measurement noise, such as the 20-day and the 16-day wave at Seoul. The quasi 16-day wave is very weak at Seoul, which is shown by the ground-based and the satellite data. The phase discrepancy at Seoul is unfortunate, as you said, but since the observed phases at the two other locations where the wave activity is high agree among ground-based and satellite data, we came to the conclusion that the phase discrepancy at Seoul is due to the very low wave activity and the measurement noise, such that there is the problem of adequately determine the phase of these weak and "noisy" waves. In the revised manuscript, we reworded the corresponding sentences according to the explanation given here.

- *Lines 314 - 315: To me the quality of ECMWF at 0.05 hPa is very questionable. Even while their output contains those levels it does not mean that the data are reliable in any way. There are essentially no observational constraints and certainly some sponge layer issues. I would feel more comfortable with analyses from the NOGAPS-ALPHA model where actually mesospheric TIMED/SABER and Aura/MLS data are assimilated. It seems even better to use the Aura/MLS GPH data themselves, even though there is a gap polewards of 82°N. Even though I surely believe that the position of the wave amplitude is related to the vortex centre, as stated in Lines 327 - 329, so far there is no rock solid evidence to support that.*
  You are correct, the ECMWF fields in the mesosphere are not constrained to observational data and need to be interpreted with care. Nevertheless, we do believe that the ECMWF geopotential height data (especially if shown as a winter-time average) are a good approximative representation of the mesospheric polar vortex. In the revised manuscript, we point out the deficiency of the mesospheric ECMWF data with regard to observational data.

- *There are some inconsistencies regarding the digital object identifier doi. Sometimes they are there, sometimes not. I guess that will be fixed later by Copernicus in the final production stages.*
  We tried to fix these inconsistencies in the revised manuscript.

Typos:

- *Line 181: "North" should read "north".*
  Done

- *Line 333: "… related with the …" should read "… related to the …".*
  Done