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Interactive comment on "First measurements of tides in the stratosphere and lower mesosphere by ground-based Doppler microwave wind radiometry" *by* Jonas Hagen et al.

Anonymous Referee #1

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General comments

This is an interesting manuscript dealing with the extraction of daily tidal signatures in winds in the stratosphere and lower mesosphere from ground-based Doppler MW measurements. The results are novel, of high interest to the community and the manuscript is in principle suitable for ACP. I do have several comments, some of them not only minor that – in my opinion – should be addressed before the paper is published.

My main concern is, whether the intermittency found in the analysis of the tidal parameters over the 7-day or 13-day time scales is real. As displayed in Fig. 2, the day to day wind variations do not really show an obvious diurnal tidal signature, which suggests

that averaging over extended periods of time is required to suppress the "noise" and to identify the tidal signature with high significance. I would like to point out that I do not question the presence of the tidal signature in the MW data set in general. The analysis of the 3-month periods is quite convincing in my opinion. But the analysis does not rule out the possibility that the relatively strong intermittency in tidal parameters is in fact due to "noise". I make some specific suggestions how to address that and which parts of the paper should be adjusted somewhat. Some of the conclusions drawn are not fully justified in my opinion.

Specific comments

Page 1, line 4: "Current lidar and satellite techniques measure atmospheric tides only in the temperature field and continuous measurements of the tides in the wind field of the stratosphere and lower mesosphere are not available."

This statement is not entirely incorrect, but existing Doppler lidar mesaurements in principle allow studying tides in atmospheric winds, too. These measurements have been used, e.g. to investigate GW signatures in middle atmospheric winds (Baumgarten et al., Geophys. Res. Lett., 42, 10,929 – 10,936, doi:10.1002/2015GL066991). I admit these measurements are not continuous over longer periods of time.

Page 2, line 23: "evident above approximately 40 km altitude and the spread between them is quite large in the lower mesosphere."

It would be good to provide some values here.

Page 2, line 32: "presents" -> "presented" ?

Page 2, line 34: "complimented" -> "complemented"

Page 3, line 28: "the Doppler shift introduced to the emission line is directly proportional to the wind speed"

Only the line of sight wind can be measured, which perhaps should be mentioned. If

zonal and meridional winds are measured, then the radiometer must allow for different viewing directions. I suggest discussing this aspect of the instrumental setup in one or two sentences.

Page 5, lines 4 – 9: the approach used here is essentially a "composite analysis“ or "superposed epoch analysis“. Perhaps you can mention these terms (or one of them), because the readers will probably be more familiar with these terms than with "aggregation scheme“

Page 6, line 16: "In this study, we do not apply any vertical smoothing to the reanalysis data.“

Why not? The reanalysis data could easily be smooted with the radiometer averaging kernels. I suggest doing that. It will affect the results and it can be easily implemented. So, why not doing it?

Page 6, line 19: “We further estimate the uncertainty of the amplitude and phase for the 3-monthly mean using a bootstrapping method by re-sampling the wind time-series“

How is the resampling done? Is it entirely random? There are many different ways to do that and the detailed approach chosen will directly affect the uncertainty estimates and/or the significance of the results. Please provide a brief description, how this was done.

Page 7, line 10 and Figure 1: I think you didn’t explain what you mean by "background winds“? Perhaps I missed. It would also be good to mention what the temporal resolution of the background winds is – probably 1 day?

Page 7, line 16 and Figure 2: In my opinion Fig. 2 does not convincingly demonstrate that the measurements capture the diurnal tidal signature. The "noise“ (or oscillations, natural variability etc.) in the time series is (are) quite large. Apart from showing the time series over a period of 7 day, I suggest also showing a plot of the superposed tidal signatures, i.e mean diurnal variation averaged over 7 days or an extended period of

time. A composite analysis will suppress the noise and show, whether a tidal signature is visually present in the data. In addition, the superposed tidal signature should also be shown for the 3-month period. I imagine that this plot reveals a remarkably clear tidal signature. This would strengthen the paper significantly in my opinion.

Page 7, line 24: "Further, our observations reveal an intermittency of the diurnal tidal amplitude at the resolved temporal scales of 7 to 13 days“

Looking at Figure 2, I wonder whether this intermittency is real or an artifact. I’m not really convinced all the signatures attributed to the tide are actually caused by it. This intermittency may also be "noise" in the data and not real atmospheric intermittency.

Fig. 3a: Please comment on the vertical structure seen in the amplitude A1. Is this a retrieval artifact? Visually it reminds me of oscillations in the profile retrieval, but it certainly may have other causes.

Page 7, line 28: "is not reproduced in the reanalysis neither in amplitude nor in phase“

Again, the intermittency in the data may not be real.

Page 8, line 4 and Figure 4: red symbols missing in Fig. 4b)

Page 8, line 3: "The green color shows the MERRA-2 reanalysis applying the same temporal aggregation of the time series to derive the tidal amplitudes as for WIRA-C, whereas the red color shows the analysis without averaging.“

This description is somewhat different from the description in the caption. The caption mentions smoothed and unsmoothed MERRA data. Perhaps you can use the same description in the text and the Figure caption.

Page 8, line 9: "wavelength of 30 km“ -> "wavelength of about 30 km“ ?

Page 8, line 10: "In both data sets, the vertical wavelength increases drastically above 55 km altitude, and the phase eventually becomes constant with altitude.“

Well, most of the data points are missing for MERRA at altitude above 55 km, so I'm not sure this conclusion is justified for MERRA.

Page 8, line 17: "The shaded area represents the statistical uncertainties of the estimated diurnal tidal amplitudes"

As mentioned above, the way in which the uncertainties are determined should be explained in more detail. There are many possibilities to implement a bootstrapping technique and any uncertainty can be obtained (that's of course a bit exaggerated).

Another question about Fig. 5. The plot shows time series with 1 day resolution. I guess you are showing the results for the 7 day or 13 day analysis at the center day, right? This should be mentioned explicitly and also, whether the 7-day or the 13-day analysis is shown.

Page 9 and Fig. 7: also for the Arctic case discusssed here, the measurements show much more variability and intermittency than the reanalysis. And the agreement between tidal parameters extracted from measurement and model is very good if the full 3 month period is analysed, as shown in Figure 8. This good agreement is probably caused by better "noise" suppression. Again, I suggest performing a composite analysis for the different time periods. The 3-month averaged results seem to be rubust, but I'm not convinced the intermittency is real.

Page 10, line 8: "phase, indicating the presence of highly intermittent diurnal tides."

or just "noise"? Noise should be excluded before drawing the conclusion you drew.

Page 10, line 18: "This is not the case for the measurements and we conclude, that the coherence time of short time scale disturbances is longer in reality than in the reanalysis model."

Again, I think noise should be excluded as a potential explanation before drawing this conclusion.

Figures 4 and 8: please also show uncertainties of the phase values.

It would also be good to show a plot like Figure 2 for the Arctic results.

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