

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 #2

Received and published: 18 October 2019

The authors describe a novel method of retrieving atmospheric tides from the Doppler microwave wind radiometer observations. The new technique is important for the atmospheric research community due to the limitations of existing ground instruments, lidars and radars, in measuring tides in the range of altitudes from 40 to 70 km. The authors used an original retrieval method for the diurnal tidal components, and compare the retrieved tidal amplitudes and phases with MERRA-2 reanalysis. I believe the manuscript could be published after a revision. I have the following major comments. First, the authors stress the importance of non-zero wind a priori for the retrieval. However, I do not see a clear conclusion of how the non-zero a priori impacts the retrievals. Second, the comparison with MERRA-2 reanalysis requires more details (see comments

C1

below). Third, the authors should comment on the applicability/limitations of radiometer observations for the retrievals of shorter period tides (semidiurnal, etc.) and other oscillations. More specific comments are below.

Page 1

Line 3: “up to the thermosphere” would be more relevant.

Line 4: “they are gravity waves” is confusing. Perhaps “planetary scale” or “global scale gravity waves”, to distinguish from small scale gravity waves?

Line 4: Satellite techniques also measure wind fields associated with tides, though under certain limitations, please clarify.

Page 2

Lines 9-10: This statement does not reflect the current state of knowledge. Various methods have been utilized to extract short-term variability of tides from satellite observations. A good overview of these methods is given by Ortland, JGR, 2017, doi:10.1002/2016JD025573.

Line 21: RMR lidars can measure winds as well as temperatures in upper stratosphere / lower mesosphere. I assume the authors mean that lidars are not particularly suitable to study tidal oscillations. This needs to be clarified.

Page 3

Line 3-4: This sentence is out of place and should be (re)moved. The radiometer should be introduced first.

Page 5

Line 24-33: I believe this method of tidal decomposition has been applied before, also by the authors of this study, e.g., Stober et al., 2017; McCormack et al., 2017. References to the earlier works are needed.

C2

Page 6

Line 7-9: This needs to be further discussed. Basically this requires some stationarity, both in tidal amplitude and in phase. Perhaps the limitations of retrievals should be also discussed in the Summary section.

Line 31: I am not sure if the chosen interval for Andenes campaign satisfies the proposed criteria. Stronger planetary wave activity starts already in early November.

Page 7

Line 10-11: I do not understand the term “composite” here. Do the authors refer to superposed epoch analysis? If they simply refer to complementing WIRA-C with MERRA-2 data, it is better to avoid the “composite” term. More importantly, the authors should detail how the WIRA-C data are complemented with MERRA-2. The representation in Fig. 1 is not clear. When the hatched area goes to higher altitudes (e.g., in meridional winds) – does this correspond to gaps in the WIRA-C dataset?

Line 17 and Fig. 2: The text says “approx. 53 km” but the Fig. 2 capture says “approx. 52 km”.

Line 31-32 and Fig. 3-4: Fig. 3 shows, the short term variability is not at all reflected in the reanalysis. From Fig. 4 we can see that the original and smoothed reanalysis show very similar mean behavior (which is not surprising). How would Fig. 3 look if the non-smoothed MERRA-2 reanalysis is analyzed?

Page 8

Line 14-15 and Fig. 4 a-b: It is surprising to see good mean agreement in zonal component behavior, but poor agreement in meridional above 55 km. Would be useful if the authors add a plot of phase differences between zonal and meridional components as a function of altitude, similar to Fig. 4b, but only the phase differences.

Line 16: Again a mismatch: the text says 55 km altitude and the figure capture says 53

C3

km.

Page 9

Line 6-11 and Fig. 7: Again, the smoothed reanalysis does not reflect the short term variability. How would the amplitudes of non-smoothed reanalysis look?

Line 17-21: Again I would suggest to plot the phase differences as a function of altitude.

Interactive comment on *Atmos. Chem. Phys. Discuss.*, <https://doi.org/10.5194/acp-2019-870>, 2019.

C4