

Comments on the ACPD manuscript entitled "First detection of tidal behaviour in polar mesospheric water vapour by ground-based microwave spectroscopy" by Kristofer Hallgren and Paul Hartogh.

The present paper describes the detection of diurnal variation in water vapor due to tidal waves in the Arctic mesosphere based on ground-based radiometer measurements. Observations of the diurnal variation in mesospheric water vapor are very rarely. There is a small number of satellite observations that potentially could provide information on the global scale, however no data have been published so far. The only publication is based on ground-based radiometer measurements in the mid-latitudes. The manuscript is therefore absolutely relevant to ACPD/ACP as it discusses a unique data set that fills an important observational gap. However the manuscript seems kind of rushed, unpolished and at times not particularly well organized. A number of things can be significantly improved and expanded. Therefore I recommend a publication only after revisions. Please find my comments below.

Section 1 - Introduction:

In general:

The introduction should be expanded for a better guidance of the reader. How does the water vapor distribution in the mesosphere look like? Why is it actually that the diurnal variation of water vapor is determined by dynamics and not by photochemistry? (It said later in the text, but too late.) How are tidal waves forced in the atmosphere? What were the main findings of Haefele et al.?

Page 31267:

- Lines 4 and 5: "high temporal resolution" - How is that meant? With regard to the measurement repetition rate during a certain period of time? For a given location I would not consider the temporal resolution of satellite measurements as high.
- Lines 11 to 13: Thermal tides were also observed by the MLS instrument aboard UARS, covering the altitude range between 20 km and 85 km (Forbes and Wu, 2006).
- Lines 22 to 24: For any species to be a dynamical tracer its chemical life time needs to be in a similar order to the characteristic dynamical time scales of

interest (Brasseur and Solomon, 2005). If the dynamical time scales are much shorter than the chemical life time of a species this species will become well-mixed, gradients disappear and, hence, tidal waves could not create any diurnal variations. Since the chemical life time of water vapor is in the order of days to weeks in the mesosphere its diurnal variation is dominated by tidal waves.

Section 2 - Instrument:

Page 31268:

- Lines 7 and 8: “for some of the mentioned papers above” - Does this refer to the publications on page 31267 in lines 19 - 21, or to the subsequent list? If it refers to the subsequent list it is worthwhile to mention what has been studied with the old instrument data, like the annual variation, sudden stratospheric warmings, the quasi 5-day wave, QBO signatures or decadal trends at ALOMAR.
- Lines 24 and 25: I suggest to remove this sentence as it begs questions about consistencies and because the time series is not the topic of this manuscript.

Page 31269:

- Line 22: “moderate” - This is maybe not an appropriate word here. I guess the point here is to make clear that the instrument has good sensitivity even at low temperatures.

Section 3 - Data retrieval:

Page 31270:

- Lines 5 to 7: Can you elaborate on how this is done. To my knowledge the Luebken climatology does not cover the entire year.

Section 4 - Observations and analysis:

In general: Some parts of this section would fit better in section 5, the discussion.

Page 31271:

- Lines 1 to 6: The data integration method seems to remove the advantage of the new radiometer system that is advertized here. You could have done that with the old system as well, of course resulting in poorer error statistics. 180 h of integration is still quite a lot. It is also somewhat contradictory to what is, at least, implied in lines 22 - 24 on page 31269. To which altitude can you now retrieve water vapor reliably with a 6 hour integration under normal conditions? T_{start} and t_{int} should be properly defined or replaced by text.
- Line 19: Why is this Fig. 4? This seems to be an obvious Fig. 2, also from the flow of the manuscript.

Page 31272:

- Lines 4 and 5: Did you not set any particular measurement response threshold to limit the influence of the a priori?
- Lines 11 and 12: Can you expand on the results of Charles McLandress' paper. By itself it is not explained very much. I would also recommend to move this to the discussion section.
- Line 16: I highly recommend to show the phases.

Technical comments:

Page 31268:

- Line 21: "was" should read "were".

Page 31270:

- Line 22: "Fig. (1)." - Remove parentheses.

Page 31273:

- Line 19: "((Zhang et al., 2010a,b))." - Remove double parentheses.

Page 31275:

- Lines 25 and 26: “mesopause region at Arctic and Antarctic latitudes” should read “mesopause region at Antarctic latitudes”.

References:

Forbes, J. M. and Wu, D.: Solar Tides as Revealed by Measurements of Mesosphere Temperature by the MLS Experiment on UARS, *Journal of the Atmospheric Sciences*, 63, 1776 – 1797, doi:10.1175/JAS3724.1, 2006.

Brasseur, G. and Solomon, S.: *Aeronomy of the middle atmosphere*, Springer, ISBN-10 1-4020-3284-6, P.O. Box 17, 3300 AA Dordrecht, The Netherlands, 2005.