Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2017-191-RC1, 2017

Interactive comment on "Variability of the Brunt-Väisälä frequency at the OH*-layer height" *by* Sabine Wüst et al.

C. von Savigny (Referee)

csavigny@physik.uni-greifswald.de

Received and published: 20 July 2017

Dear Christian,

Thank you very much for your valuable comments. I tried to include them as best as possible.

One thing I realized during the revision of the manuscript: On page 3, line 30 (original manuscript), I wrote "Error propagation shows that an error of 10% in the BV frequency leads to an error of 20% in the density of wave potential energy (see Wüst et al., 2016)." This mentioned calculation was included in the first version of Wüst et al. (2016). Due to re-arrangements of the manuscript in the review process, I deleted it. Therefore, I now included the calculation in this manuscript and deleted the reference to Wüst et al. (2016).

General comments:

This is a generally well written study on the variability of the Brunt-Väisälä (BV) frequency in the MLT region. Knowledge of the BV frequency is relevant for the derivation of gravity wave related parameters, e.g. from ground-based observations of MLT temperature fluctuations. The results presented are useful for the aeronomy community and particularly for the groups operating ground-based OH rotational temperature spectrometers. I have no major objections against the publication of this manuscript, but ask the authors to consider the specific comments listed below.

Specific comments:

- Page 1, line 16: "which are" -> "which is" Done
- Page 2, line 25: "The same holds for the BV frequency" It's not clear, what "The same" refers to. Please rephrase. Done
- Page 3, line 25: I suggest mentioning the factor 2 pi in the context of BV period and BV frequency. I think the formula/values are not entirely consistent. Often the factor 2 pi is already included in the definition of the BV frequency. It should be clear, whether "frequency" refers to "angular frequency" or not.

  I inserted the following sentences after formula (3) "This formula refers to the angular BV frequency. Even if not explicitly mentioned in the following, the terms BV frequency or BV period always denote the angular values." Furthermore, I included $2\pi/N$ after BV period (former page 3, line 25).
- Page 6, equation (4): I'm not sure the normalization by the norm of vector f is correct. One should divide by the sum of all elements of vector f, right? The

norm, however, has a very different value, i.e. the square root of the summed up squared vector elements - at least according to the standard definition.

You are right, the calculation is correct but the formula is wrong. I corrected it.

This probably only affects equation (4) and not the actual calculation of the OH* equivalent BV frequencies?

- Page 6, line 5: Regarding the OH* layer height: If I understand correctly, the layer height is simply the height grid point with the maximum VER, right? Yes, that's true

It would be better to use centroid altitude, i.e. altitude weighted with the VER profile. If the altitude with maximum VER is used, the altitudes will be affected by the vertical sampling of the SABER limb measurements and by the retrieval altitude grid. I assume, the effects will be very small, though, but it would be good to motivate, why the height of the VER maximum is used here.

I analysed the first half of the year 2004. This year was arbitrarily chosen. The mean difference between the centroid altitude and the peak altitude is ca. 0.7 km, the skewness of the VER-distribution is 0.8 which is not a very large value.

Since the vertical resolution of the SABER data is ca. 300–400 m, a difference of 0.7 km corresponds to 2 data points at maximum. Taking into account the FWHM of 7–8 km of the OH*-layer and the calculation method of the climatology of the Brunt-Väisälä frequency (least squares fit to the daily mean values of the Brunt-Väisälä frequency), I would judge the effect as negligible.

I inserted in the manuscript: "The assumption of a Gaussian-shaped OH*-layer is certainly simplified. In most cases, the OH*-layer follows a slightly asymmetric form with a positive skewness. That means the centroid height is a little bit higher (for example, ca. 0.7 km averaged over the first half of the year 2004) than the height of the maximum VER. Due to these small differences and the averaging which is applied afterwards to the Gaussian-weighted squared BV frequency, this simplified approach can be justified."

Also: the OH VER profile is not Gaussian. Assuming a Gaussian will also affect the results somewhat. I think you should at least mention that the actual VER profile is not Gaussian.

See above.

- Page 9, line 11: "For ENVISAT [..] on board of SCIAMACHY" –> "For SCIAMACHY [..] on board of Envisat" SCIAMACHY is the instrument, Envisat the satellite. Done.
- Page 9, line 15: Regarding the agreement between SCIA and SABER OH emission altitudes:

  Centroid altitude and altitude of maximum VER may be quite different (up to 2 km, I reckon), because the OH VER profile is asymmetric. Centroid altitude will be systematically larger than the VER-max altitude

  Remaining tidal effects between the average SABER local time and the SCIA local time (between 21 and 22 at 40 – 50 N) may also contribute to differences

  The vertical shifts between the different Meinel-bands may also play a role So, considering these differences, the agreement is quite good.

  Thank you for this hint. I mentioned it in the manuscript "In contrast to our analysis, von Savigny (2015) refers to the centroid altitude, while we show the altitude of maximum VER. These values differ, if the OH VER profile is asymmetric. Furthermore, remaining tidal effects due to different overpass times of both satellites and vertical shifts between the different Meinel-bands may also play a role. So, considering these possible sources of inconsistencies, the agreement is even quite good."

- Page 9, line 25/26: The linear trend in OH height is interesting and fairly consistent with a trend determined in our recent paper (Teiser & von Savigny, Variability of OH(3-1) and OH(6-2) emission altitude and volume emission rate from 2003 to 2011, JASTP, 161, 28-42, 2017). In this study, the trend in OH(3-1) centroid altitude (averaged between 5S and 30N) is about -20 m/yr. Higher northern latitudes are not covered, unfortunately. And one has to be careful, because trends in the SCIAMACHY limb pointing data may also play a role at this level. It is, however, interesting to note the qualitative and quantitative agreement between the different results.

  Indeed, that's interesting and I included it therefore in the manuscript p. 12, ll.6–8 (version with changes marked).

- References: The list of references contains several inconsistencies and typos, i.e.: spacing between initials is not consistent, e.g., "R. A." vs. "C.J."; in several cases the hyphen is missing between "Sol." and "Terr." for JASTP papers; in some cases there are periods between paper title and journal name, rather than commas.

  Page 12, line 23: delete extra space in "T. ," Done.

  Page 14, line 19: delete extra space in "OH (3-1)" Done.

  Page 14, line 2 bottom-up: delete extra space in "O (1S)" Done.

  Page 14, last line: comma after paper title missing. Done.

  I checked the whole reference list for inconsistencies and hope that I could identify all.