

Interactive comment on “Variability of the Brunt-Väisälä frequency at the OH-airglow layer height at low and mid latitudes” by Sabine Wüst et al.

Anonymous Referee #2

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OH airglow spectrometers can provide information about atmospheric temperature and its variability from the height of the airglow layer, which is roughly centered around 87 km. In order to evaluate gravity wave parameters, the gravity wave potential energy density (GWPED) is the more meaningful parameter. GWPED calculation requires the knowledge of Brunt-Vaisala-frequency N , which can not be directly derived from airglow data. The paper by Wüst et al. provides a data set of BV-frequency on a nightly mean basis for a broad latitude range including low and mid latitudes based on a temperature climatology by TIMED/SABER. Annual and semiannual variability of N are quantified, and reasons for higher-order variations are discussed. By intention, due to the choice of methods, the paper is solely relevant for evaluation of OH airglow data. Neither

C1

information about the “true” N around 87 km nor information about other airglow layer heights is provided. However, the paper will be of interest for a large community. I have several comments on the structure of the manuscript as well as on the depth of the results that I like to see addressed before the paper can be published.

Major comments:

- A major concern is the missing analysis of tidal effects of the results. Fig. 3 shows that SABER observes at a local time that is slowly precessing with time. That means that for a number of consecutive days the measurements happen at a distinct phase of the tides, producing a systematic offset of temperature and temperature gradient compared to the nightly mean. This becomes obvious when the data flip from ascending to descending node or vice versa. The authors somewhat discuss this effect for the apparent 60-d-variation of N , but I think the different potential biases of the results due to tidal waves needs to be further elaborated.

- P7L20-P7L25: I agree with the authors that R^2 is very low in these cases. As a consequence, the linear equation explains only a very small fraction of the relation between temperature (or T gradient) and local time. I wonder, why I should expect a linear relation at all. A sinusoidal relation due to tides might be even more likely. Even if R^2 is such small, in Fig. 4e a very precise linear local time dependence of N evolves. I would like to see some more discussion of this, from my point of view, surprising result presented in Fig. 4e. Minor topic: Please add an “x” in the best fit equation for 45°N in Fig. 4e.

- P9 (Harmonic Analysis): What is the reason for allowing the fit two arbitrary frequencies between 180 and 366 d? The authors argue that they fit an annual and semi-annual variation (which is reasonable), but the periods deviate partly by some tens of days from the particular annual/semi-annual periods (see tables 1-3). For example, in Table 1 at -35° latitude a period of 256 days (side note: I think, two decimals are far beyond physically reasonable) is given for the annual variation at 50°E , while in

C2

the next longitude bin 229 days is interpreted as semi-annual. Furthermore, partly the period of one of the oscillations is exactly 366 d or 180 d for several adjacent regions. This seems to be somewhat artificial, like if the fit would “prefer” some period outside the limits. I suggest fitting some fixed periods of 183 days and 366 days for the whole data set. Regarding the results of the harmonic analysis, the authors acknowledge a low quality of the fits at low latitudes. Unfortunately, they focus in the discussion mainly on the longitudinal differences. Please discuss the consequences of the low quality in more detail. How representative is the climatology if the fit quality is low? I further suggest showing the fit results together with the original data for at least one or two representative examples.

Minor comments:

- P5L15: I see the range “60°S to 60°N” somewhat misleading and overambitious, if true whole-year data coverage is only between 52°S and 52°N. P5L10 says that data refer to the mid of the interval, i.e. 60°S would mean 55°S-65°S. Even if the last interval is centered around 55°S/N, as Fig. 2 suggests, this interval would in fact only contain data of 50°S/N-52°S/N.

- P6L28: What is the reason for giving a percentage variation of the OH layer height? How can I interpret a 1% change?

- P6L33: As mentioned above, the yaw cycle affects data coverage already poleward of ~52°.

- P7-8: I suggest making the structure of the results sections more obvious to the reader. E.g., I realized quite lately that a large fraction of pages 7 and 8 explains the reason for the 60-d-oscillation as an artefact of the yaw-cycle in relation with tidal variations. Sub-section headings, e.g., could help the reader to follow the line of arguments.

- P7L32-33: I am sorry, but I do not understand this argument. Additionally, I suggest removing the brackets around “the mean vertical wavelength ...”.

C3

- P8L9-10: I suggest adding some text that tides not necessarily always have stable phases (there is a lot of observational evidence for “unstable” tides). But in this case, arguing for potential artefacts, the chance of stable phases is sufficient.

- P8L11: I suggest removing “However”, as the following sentence is not in contrast to the previous.

- P8L17: I am sorry, but I do not understand why you can conclude, “a semi-diurnal tide must be present”. Please try to improve.

- P9L2: If I understand the paper correctly, you may add “and, therefore, ignore the 60-d-oscillation in our BV climatology.”

- P9L28: What is the measure for the quality of approximation? R^2 , again?

- P10L14: The 5°x7° pixel has less than 25% of the size (area) of the 10°x20° pixel.

- Fig. 3: I suggest using different open and filled symbols. The two gray colors are hard to distinguish. (Please apply to Fig. 5 and 6 accordingly.)

- Fig 4: The caption gives the wrong color coding for Subpanel e).

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C4