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Atmospheric Measurement Techniques
Discussions

Answer to

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.

We would like to thank the anonymous reviewer for the valuable comments. Our answers are inserted in orange in the text below.

From the page and line references in the minor comments I conclude that the reviewer used the original version of the manuscript and not the one after the quick review. The changes during the quick review process were marginal in most parts of the manuscript (most of changes referred to section 2) and the points on which the reviewer comments are still part of the manuscript, so I just answered them and changed the manuscript where needed.

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 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.

I included an approximation of the uncertainty in N during one night due to tidal effects in the discussion: "As mentioned above, tidal effects are not included in the approximation of the OH*-equivalent BV frequency. However, an uncertainty range of the OH*-equivalent BV frequency due to these effects is an additional useful information. In order to estimate it, SABER profiles which refer to the same night (between 6 p.m. and 6 a.m.) and are separated by six hours at minimum are collected for all years (2002–2018) at each grid point. Since tides have periods in the range of six hours and more (the dominating ones have periods of 12 h and 24 h), we argue that the difference of the OH*-equivalent BV frequency within these hours during one night is mainly due to tides. Of course, also gravity waves still play a role in this period range. Since our gridding is relatively coarse for gravity waves, we assume that gravity waves increase and decrease the OH*-equivalent BV frequency in one pixel so that their effect cancels out over time. This is not true for larger scale phenomena. The mean difference per hour over all years is calculated at each grid point. The results are averaged over latitude afterwards (table 2). The results are averaged over latitude afterwards (table 2). The number of data per latitude is in the range of some 10 000. As mentioned above, tidal activity varies during the year. Therefore, the provision of monthly values would make more sense. However, especially at mid and high latitudes, SABER profiles which refer to the same night separated by six hours at least are not evenly distributed over the year or not available at every month. The uncertainty range provided in table 2 can therefore be regarded as a rather rough estimate. With respect to an OH*-equivalent BV frequency of 0.02 1/s, the results are in the range of ca. 1–2%. For a night of twelve hours tidal effects sum up to ca. 21 % at maximum (that means for low latitudes). We can assume that the approximation of the OH*-equivalent BV frequency refers to midnight, so the tidal effects can be approximated by ±11 % for the whole night in this case." This procedure is described in the section results and discussion on page 9.

Furthermore, I re-arranged the discussion of the 60d oscillation.

P7L20-P7L25: I agree with the authors that Rˆ2 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 Rˆ2 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. I inserted some discussion concerning these points. Figure 4(e) shows that even in the case that the vertical temperature gradient and the temperature were behaving strictly linearly (so decrease or increase linearly with time), the BV frequency could stay constant.

Minor topic: Please add an "x" in the best fit equation for 45°N in Fig. 4e.Done

P9 (Harmonic Analysis): What is the reason for allowing the fit two arbitrary frequencies between 180 and 366 d? The reason is that we would like to achieve the best approximation and this can be done if the frequencies are chosen freely by the analysis. The analysis was allowed to search for two periods 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). We do not fit an annual and semi-annual oscillation, we make the analysis search for the two oscillations between 180 d and 366 d which approximate the BV frequency best. In the majority of cases, the analysis finds approximately an annual and a semi-annual variation.

For example, in Table 1 at -35° latitude a period of 256 days (side note: I think, two decimals are far beyond physically reasonable changed also in table 2 and 3, which are now table 3 and 4) is given for the annual variation at 50° E, while in the next longitude bin 229 days is interpreted as semi-annual.

I deleted the part in the text where annual and semi-annual are associated with the first and the second fitted oscillation.

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. I inserted the case you mentioned above for (-35°N, 50°E) in the paper (new figure 5). I think it becomes clear that the combination of oscillation (256 & 180 days) is chosen by the analysis since the autumn maximum is flatter than the one in spring. I can repeat the analysis for fixed periods if you wish but I would like to know your opinion about this approximation.

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?

It seems that the order of sentences we chose was not optimal. The discussion is about the latitudinal variation of the quality of approximation and the reasons for it. The longitudinal variation is only mentioned in one sentence. I shifted the sentence referring to the longitudinal variation to the end of this paragraph, in order to improve the structure.

I further suggest showing the fit results together with the original data for at least one or two representative examples. I provided these examples in the new figure 5. I chose the one which you mentioned above (-35°N and 50°E) and one mid-European one.

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. Changed 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. Changed in the figure caption and also in the tables.

P6L28: What is the reason for giving a percentage variation of the OH layer height? How can I interpret a 1% change? The sense behind the percentage variations was only to put the variations in a context. For example, a variation of 0.001 1/s in the BV frequency does not sound much, but is a 5% effect relative to the mean BV frequency. I can leave it out. At the moment, it is still in and I clarified that this percentage value is calculated relative to the mean value of the respective variable.

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

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. Ok, done.

P7L32-33: I am sorry, but I do not understand this argument. Additionally, I suggest removing the brackets around "the mean vertical wavelength . . .". I removed the brackets and re-formulated the argument: the influence of waves on the temperature gradient depends on the vertical wavelength (the larger the wavelength, the less the influence for wave with the same amplitude) and the amplitude of the wave (the larger the amplitude, the greater the influence for waves with the same vertical wavelength). Additionally I made some changes in the following text.

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. Thank you for this correction. I toned down the statement and provided further information about the phase variations in other publications.

P8L11: I suggest removing "However", as the following sentence is not in contrast to the previous. Corrected.

P8L17: I am sorry, but I do not understand why you can conclude, "a semi-diurnal tide must be present". Please try to improve. I re-arranged this part of the manuscript due to the comments of both reviewers on tidal effects. Doing this, I realized that the argument I gave is only valid if the tide either influences the temperature gradient or the temperature. If both variables are influenced it will not work. Since I think that the argumentation is not necessary after the re-arrangement any more I would prefer to leave it out.

P9L2: If I understand the paper correctly, you may add "and, therefore, ignore the 60-d-oscillation in our BV climatology." Done but I changed "in our BV climatology" to "for our climatology".

P9L28: What is the measure for the quality of approximation? Rˆ2, again? No, it is not R^2. It refers to the harmonic analysis and is calculated as follows: $1 - {\sigma_{res}}^2/\sigma^2$ where σ^2 is the variance of the original time series and ${\sigma_{res}}^2$ is the variance of the residual time series, so original one minus approximation. I gave this information in brackets in the manuscript.

P10L14: The 5°x7° pixel has less than 25% of the size (area) of the 10°x20° pixel. That is also true and I changed it.

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. I changed it for figure 3 and 5, in figure 6 only one gray color is used, so nothing to change.

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