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Interactive comment

Interactive comment on "Derivation of horizontal and vertical wavelengths using a scanning OH(3-1) airglow spectrometer" *by* Sabine Wüst et al.

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We would like to thank Alan Liu for his valuable comments. We answered all of them and changed the manuscript where we think it is necessary. Please find our list of answers and comments below.

Due to the comments of all three reviewers, I made the following general changes in the manuscript:

- The calculation of the vertical wavelengths from SABER data was limited to

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one wavelength for each profile in the range of the vertical wavelength derived from GRIPS +/- the error. As I re-calculated the approximation for the height range 70–90 km (instead of 60–80 km) for comparison reasons, I found out that the original approach might deliver not the best results. The SABER profiles show two to three waves. If I restrict the adaption to one more or less specific oscillation, which might not be the dominant one, the harmonic analysis provides a kind of compromise between both waves. Therefore, I provided less restrictions to the harmonic analysis: it searched for two oscillations with a wavelengths between 2.5 km (minimal vertical wavelengths detectable in SABER measurements according to Trinh et al., 2015) and 20 km (height interval length) and I used the one which fits better to the GRIPS vertical wavelength. Applying this approach, the difference between the vertical wavelengths derived from both approaches halves.

- When adding additional information to former table 2, I found out, that I included one wave with a rather long wavelength (33 km) in the subsequent analysis. This is not consistent with the exclusion of waves with vertical wavelengths longer than 20 km. Therefore, I corrected it.
- I used the Brunt-Vaisala frequency calculated directly from the SABER profiles

These leads to different figures compared to the previous version. However, the main message of the paper does not change.

This work is an analysis of gravity wave horizontal and vertical wavelength based on a spectroscopy measurement of OH vibrational temperature at 4 directions, 3 off-zenith and 1 at zenith. The horizontal wavelengths were derived based on phase differences among different directions. The vertical wavelength was derived based on gravity wave linear theory, and a nearby meteor radar wind measurements,

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together with assumed climatological Brunt-Väisälä frequency. The derived vertical wavelength was also compared with those derived from SABER temperature profiles. Error analysis of vertical wavelength was performed based on estimated errors of other parameters in the gravity wave dispersion relation.

The major additional work from a previous 3-direction measurement is to the derive vertical wavelength. This, in my opinion, does not provide any useful information. The derived vertical wavelengths have large errors, and have no consistency when compared with SABER. The measurement of a single airglow layer temperature can perhaps derive horizontal wavelength and period, when done very carefully, but it does not actually provide any information on the vertical wavelength. The inferred vertical wavelength is critically based on wind measurement elsewhere, and the dispersion relation. The major contribution from the OH measurement in addition to the wave period is the horizontal wavelength, which has already be published. Hence, I do not see value in publishing this work. If the authors like to improve on the current work, I'd recommend addressing on the improvement that a 4-direction measurement can make over the previous 3-direction measurement. In the following I list several major problems with vertical wavelength derivations, which I think cannot be mitigated because it's intrinsic to the limit of the measurements.

The mean difference of 4 km (Fig.4), over a mean vertical wavelength of 12.5 km (Fig.2) is quite large. The fact that the OH vertical wavelength has an error of 59%, ever larger than the mean difference from SABER (at 41%) means the comparison with SABER is meaningless. The difference cannot possibility be smaller than the error. It shows that these values are purely incidental. The estimation of the error is based on error propagation calculation. Here, the uncertainties of the different parameters contribute. Even if done very carefully, these uncertainties are still estimates. The comparison of the vertical wavelengths based on the spectrometer radar combination to the vertical wavelengths derived from SABER shows that the provided error is in the same order of magnitude but might be too conservative. We changed “A mean error of 59% as calculated based on error propagation can be therefore regarded as a conservative estimate.” to “The vertical wavelengths agree within the error bars in all but four cases; here, the vertical wavelengths derived from SABER are slightly smaller by 0.2–0.8 km.”

page 9: The possibility that the intrinsic frequency is very close to f is not addressed.

It does not need to be addressed since it is not the case. It can happen that the intrinsic frequency and f are similar, however, these values are excluded from further analysis since the vertical wavelengths are rather small (as described in section 4.2).

Since the uncertainties in u and v is 20 m/s, and the measured phase velocities are mostly between 20-40 m/s (10,5-6), some waves could have very small intrinsic phase speed and frequency. When this happens, all the errors in eqs.(4)-(8) will be huge, and the linear approximation in the error analysis does not apply anymore.

How will the emission height affect the derived horizontal wavelength, and thus vertical wavelength? Its effect is not considered in the error analysis. The height is known to vary, especially with tidal motion, by several km.

Such effects are of interest, if they influence the different FoV to a different extent.

Our results rely on spatial and temporal averages:

- The FoV covers 880 km² (560 km²), all values, we derive are averaged over this area.
- We analyse time series of 7 h and longer.

Furthermore, the four FoV are rather near to each other (see new figure 1 for better understanding). This reduces the possible effect of large scale motions like tides on our analysis results tremendously.

Third, due to the redundancy of the system (we get four values for the horizontal wavelength), we dismiss results which do not agree sufficiently (as mentioned in section 3.1). This might be the case when not all but only one or two FoV are influenced by a higher or lower airglow altitude.

Therefore, we judge the effect the reviewer mentions as an effect of second-order and neglect it.

page 4, line 12: Since the 4 directions were measured at different times, how do this affect the derivation of horizontal wavelength? Is the 15 s or longer lag between different directions taken into account? How does this affect the errors? As mentioned on page 2 “The instrument acquires spectra with a temporal resolution of 15 s. … In order to improve the signal-to-noise ratio for the intended analysis five minute mean values are calculated for each FoV.” Furthermore, we write on page 7 “Further analysis steps are restricted to results which are characterized by a period longer (shorter) than 60 min (the measurement time)”. Therefore, we assume that the time lag of 15 s is negligible.

page 5, line 6,11: 60-80 km temperature from SABER is used to derive vertical wavelength, but this region does not overlap with the airglow region. The reason given is that the temperature is sharply changing around 86 km in summer. This shows a problem with deriving vertical wavelength using a snap shot of temperature profile. Without temporal revolution, one cannot tell whether a sharp gradient is due to a wave or a more permanent feature. Therefore, the SABER derived vertical wavelength itself is not reliable either.

In order to analysis a temporal-spatial phenomenon such as a wave, I should ideally use data which are provided with sufficient temporal and spatial (3D) resolution. However, such data are not available. Based on this argumentation, I can never use data of orbiting satellites or of radiosondes, for example. They are always snapshots and the repetition rate of these instruments is far too low compared to the lifetime of gravity waves. Also lidar data could only be used in parts since they do not provide horizontally-resolved data. In principal, I can expand this argumentation to every instrument.

Since we are aware of the fact that the mesopause might cause a problem in detrending, we used the height range slightly below.

In addition, I repeated the analysis based on the height range of 70–90 km for our data covering September to December (=months with higher mesopause). The results look pretty similar.

Furthermore, if a large gradient does exist in the airglow region, the vertical wavelength derived below 80 km is not a reliable comparison, because waves propagating into a large N square region (a strong inversion layer below the mesopause) will change vertical wavelength.

That’s true, vertical wavelengths are always influenced by the wind field and the Brunt-Vaisala frequency which changes more or less with height. This effect weakens (averages out) when using temporal or spatial averages (e.g., due to the measurement technique or the analysis method). When I analysis the height range 60–80 km or 70–90 km and the wave changes its wavelengths there since the background atmosphere varies, I will get an averaged value, for example.

Therefore, we would never expect an ideal agreement between both data sets. We use the SABER data just as a kind of consistency check. The question we would like to answer is: do we find wavelengths in SABER data which agree with the wavelengths derived from the GRIPS-radar combination within the error bars. If we hadn’t used the SABER data, we would probably have to answer the question from another reviewer why we did not look into other data sets to check our results. That is why we present the SABER analysis here, even if we know that this combination is not ideal.

page 2, line 25: Only one OH spectrometer, is in contrast with .., but implies that no other measurements are needed. This is not true, since meteor radar wind was used, and N2 is approximated. Changed to “Here, for the first time, we present an approach to derive zonal, meridional and vertical wavelengths as well as wave periods based on only one OH* spectrometer addressing one vibrational-rotational transition and on additional information about the horizontal wind and the Brunt-Väisälä frequency.

page 4,line 10-11: not clear what the FoV size of the triangle means. They are

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several hundred km, much larger than the length of the triangle. Changed the formulation to “The edge length of the FoV triangle amounts to 90 km. Due to the finite aperture of the GRIPS 14, each FoV covers approximately 880 km^2 excluding the one in the zenith direction. The latter is smaller with ca. 560 km^2 (see fig. 1).” and added a sketch to make it clearer.

Table 2 only lists SABER vertical wavelength. Why not put the OH vertical wavelength as an additional column for direct comparison? Done

Why use monthly mean N square, not use the SABER temperature to derive N square? Done. I also inserted in section 3.2 why we still use a relative uncertainty of 10%.

page 2, line 15: ’maximum’ is ambiguous. I suppose this means ’maximum number of waves’ Yes and corrected

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