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Atmospheric Measurement Techniques Discussions

Interactive comment on "Tomographic reconstruction of atmospheric gravity wave parameters from airglow observations" *by* Rui Song et al.

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[amtd, manuscript]copernicus [parfill]parskip

Responses to reviewers' comments on "Tomographic reconstruction of atmospheric gravity wave parameters from airglow observations" by Song et al.

The authors would like to thank the reviewers for their valuable comments, which helped us to improve the quality of this manuscript. We have addressed all the com-

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ments, and the reply to each comment is highlighted in blue as follows.

Reply to Anonymous Referee #2

This manuscript thoroughly describes a methodology of retrieving 3D gravity wave parameters (wavelength and amplitude) from a synthetic remote sensing instrument that is designed to work on the "target mode" at O2 A-band. Taking the advantage of combining both "limb" and "sub-limb" strengths, this "target mode" can capture the majority of the gravity waves on the spectrum except the very small ones (both horizontal and vertical wavelengths are small). The aiming region is at mesopause where a lot of gravity wave breaking and secondary generation occur, so this methodology, together with the specially designed viewing geometry, is likely a powerful tool of investigating the gravity wave dynamics, and mesosphere-thermosphere coupling on a global scale. This paper is well-written. The flow is smooth, the logic is strict, and the presentation is concise and clear. It well suits the journal of AMT, and deserves a final publication. I have some broad questions and comments that I hope the authors can address before final publication. I don't want to hit the "major revision" button because the following comments are indeed not too critical. But I sincerely hope the authors could take at least #3 seriously and add one figure to address this issue.

We thank the reviewer for providing a thorough review and offering valuable suggestions. We have revised the manuscript according to the comments. Especially for #3, an additional figure has been used to clarify the problem.

1. Although the "observation" is synthetic, the paper is not clear about what the designed orbit, scan frequency, global coverage, etc. should be, so readers have no idea whether this "mission", if successfully launched, could be suitable for case studies and climate studies

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Thanks for the suggestion. A few sentences have been added in the beginning of Sect 5.2 to clarify this:
" The satellite platform is simulated in an approximately 600 km sun-synchronous orbit with an inclination angle of 98°. The instrument will employ a 2D detector array consisting of about 40 × 400 super pixels. It measures in the spectral regions from 13082 to 13103 cm^1 within the altitude range from ≈60 to 120 km in limb imaging measurements.".

2. Similar to the above question, the integration time of each limb/sub-limb view seems to impact the sensitivity window (i.e., Fig. 8). Other than gain (or signal-noise ratio), I don't see a clear way that they are connected. Can you quantitatively elaborate why?

Yes, the integration time will affect the sensitivity window as shown in Fig. 8. The sensitivity to horizontal waves can be increased by using shorter integration time. The shorter the integration time is, the less horizontal information of the atmosphere will be smoothed in each limb or sub-limb view. However, this integration time should also be adequate such that enough photons can be received by the instrument. In this experiment we aim to show the readers that, for the same instrument the horizontal resolution can be improved by incorporating sub-limb measurements, even if the integration time of the instrument itself can not be improved. For this reason, we didn't show the influence of integration time on the sensitivity window in Fig. 8. Then it's clear to see how the 'target mode' can improve the performance in analyzing horizontal wavelength of the waves.

3. The authors mentioned that one of the difficulty this "target mode" can conquer is that we don't need two adjacent orbits to determine the horizontal wavelength. But in my understanding, the aliasing effect still exists, i.e., the satellite instrument is still only sensitive to wave fronts that are parallel to the LOS. In the "pseudo-retrieval",

the input “truth” is also a linear gravity wave with wave front parallel to the LOS. What about other direction? I think an evaluation of the dependence of retrieved wave parameter as a function of wave vector direction is necessary to show to the readers. In addition, it would be nice to briefly discuss the situation of a mixture of two linear waves, and other types of GWs, e.g., circular rings. The general interests lie in the fact that many GWs become non-linear at the mesopause.

Agreed. In this ’target mode’, the instrument is sensitive to the wave vector that has a component parallel to the LOS. This effect was not clarified in the manuscript. We added a figure to illustrate the viewing geometry between the LOS and wave vector. A few sentences were used to clarify this effect before the results of the sensitivity study are presented:
"For this ’target mode’, the observed horizontal wavelength is the wavelength projected along the LOS. In general, there is an angle α between the LOS and the horizontal wave vector. Therefore, the observed horizontal wavelength λ_x is a factor of $1/(\cos\alpha)$ larger than the real horizontal wavelength λ_r, as illustrated in the added figure. In this sensitivity study, the horizontal wavelength discussed is the observed horizontal wavelength λ_x".
The focus of this paper is to show how well the horizontal resolution can be improved by performing the ’target mode’ observation. In the case of 2-dimensional retrieval, a combination of vertical and horizontal information is enough for the analysis of any 2-d waves. We are currently working on another retrieval strategy to get more information on the orientation of the wave vector. However, this topic is rather complex and would fill another paper.

4. Regarding the horizontal wavelength, there is still no way to decompose it to lambda_x and lambda_y, is that right?

Yes, right. This point has been clarified in Comment #3.

Minor points: P1, L3: wind system -> wind structure. P1, L15: for -> from P2, L8: they include -> these datasets include P2, L10: In Wu and Waters (1996), they used the saturated radiance (and hence, it's sub-limb technique, not limb, read Wu and Eckermann (2008, JAS) for details), not the retrieved temperature. P2, L15: Please include Gong et al. [2012, ACP] and Hoffmann et al. [2016, ACP] in the reference list. P2, L23: short horizontal waves –> waves with short horizontal wavelengths. P2, L32: add "small" before "structure". P3, L19: observation -> observations.

Thanks for the detailed reading. All these minor points have been addressed in the revised manuscript.

Fig. 5: My understanding is that this figure shows the weighting function of each channel, correct? If that's the case, I think it's better to draw the weighting function line as a function of altitude for each channel, using different color to represent different channels would be a better idea. Right now it's not straightforward of the subtle difference of weighting function peak at different altitude.

This figure is not the weighting function for each channel. It shows the simulated radiance at different tangent heights.