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Interactive comment

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

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Review comments for "tomographic reconstruction of atmospheric gravity wave parameters from airglow observations" by Song et al.

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

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Discussion paper

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.

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.

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?

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.

4. Regarding the horizontal wavelength, there is still no way to decompose it to

lumbda_x and lumbda_y, is that right?

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.

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.

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