

Atmos. Meas. Tech. Discuss., referee comment RC1
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Comment on amt-2022-224

Anonymous Referee #1

Referee comment on "Satellite observations of gravity wave momentum flux in the mesosphere and lower thermosphere (MLT): feasibility and requirements" by Qiuyu Chen et al., Atmos. Meas. Tech. Discuss., <https://doi.org/10.5194/amt-2022-224-RC1>, 2022

The authors Qiuyu Chen et al. did a comprehensive study on the use of a miniaturized spatial heterodyne spectrometer for the measurement of temperature in the mesosphere and lower thermosphere. Special emphasis was put on the use of this data for gravity wave studies and the question of whether a constellation of 2 or more CubeSat-sized instruments is sufficient to measure gravity wave momentum fluxes (GWMF) of mesoscale waves. The realization of this mission would be highly relevant for further studies of gravity waves but at comparatively low costs. The presented study combines orbit simulations, airglow modeling, temperature retrieval as well as wave analysis, and GWMF calculations. Temperature data of the high-resolved HIAMCM are used for the simulation of the measurement, and self-consistent wind data from the same model as an independent measure for GWMF. The temperature data is taken "as is", sampled with assumed orbit parameters, and added with potential instrumental noise in the different sections of the manuscript.

The manuscript is generally well written and well structured. All arguments are clearly described and reasonable, and the conclusions are justified. The authors present a very comprehensive study, including spectroscopy, instrument design, modeling, and geophysics. My main recommendation, therefore, is that the authors should keep the focus of the manuscript clearer and reduce side-topics or well-known aspects. That would help the reader to keep oversight over the 19 figures and related descriptions. I recommend a minor revision of the manuscript and provide in the following more detailed comments.

Minor comments:

- The Introduction includes an extensive description of the relevance of gravity waves for the understanding of the middle atmosphere. I think this is without doubt, and the

description can be shortened. The description of the MATS mission is from my point of view not relevant for a feasibility study of another instrument

- l. 184/185, Appendix A: As far as I understand, the described method of Ern et al. is not used in this manuscript and the comparison of methods is beyond its scope. Therefore, I suggest removing Appendix A and rephrasing this sentence.
- Fig. 2, Section 2.3: Fig. 2 is very deductive and important for the understanding of the method. I suggest referring the “first question/second question/third question” to the respective upper/middle/lower yellow diamonds and adding references to Sections 4.2 and 4.3.
- l. 505: I do not understand why another cutoff is applied to the simulations compared to the reference. Please describe.
- l. 507: It does not become clear to me whether in some cases (Fig. 10 c and f) there are no waves below 150 km wavelength, or whether this part of the spectrum is not shown for technical reasons.
- Section 4.3: I recommend referring at the beginning of this section back to Fig. 2 and Section 2.1.
- Section 5 discusses in general the relevance of the examination of mesoscale gravity waves independent from the proposed instrument or the availability of additional information (wave sources, winds, ...). I suggest either shortening this section or pointing out why these studies cannot be done with other (existing) instruments.
- l. 610 – l. 613: I agree that wind information is crucial for the understanding of wave dissipation and other processes. However, it is hardly available on a global scale in the MLT. Wind data from assimilated temperature information may lack precision, especially for non-linear processes. Effects like GW bending cannot be acknowledged at all. Please comment on the consequences of limited data availability for the science questions.
- l. 643/644: I suggest comparing to other global observations instead of comparing to models. Even GW-resolving GCMs may not display true atmospheric states despite they are good tools for the understanding of atmospheric processes.
- l. 653: I suggest describing the effects of the observational filter much earlier.
- l. 676/677: I agree with the statement about zonal mean climatologies but suggest removing the two lines including wind data. The authors describe their own concerns in L. 636-638.

Technical comments and typos:

- l. 63: “residual”
- l. 429: “shown by Lehmann et al. (2012)”
- l. 439: “(cf. Section 2.1 and Figure 2)”
- l. 450: “by Ern et al. (2004)”
- l. 470: I suggest adding “(see data flow to the middle yellow diamond in Fig. 2)”
- l. 498: “5-track”
- l. 583: “can be generated” should read “can be calculated”