Response to the comments of referee #2

We thank the referee for their review including detailed comments and suggestions. It will strengthen the output of the study. We have addressed all of them one-by-one in details as listed below. The comments are in bold and our replies are in regular font. The line numbers indicated in our replies are given with respect to the track change manuscript, and may differ from the revised manuscript.

General issues:

You absolutely cannot ignore self-absorption in the 80-85 km region. Even in the 85-90 km region it is not negligible. If you want to include this region (80-90 km), you must account for self absorption.

This comment led us to further investigation on this topic. In Fig. 1 we present the radiance distribution along the line of sight (LOS) normalized to the maximum of each LOS. The limit of 80km was previously derived by Fig. 1a. However, investigating the tangent altitudes between 80km and 90km reveals that the lower most altitudes are affected by self-absorption, where 50% from the radiance come from the strong signal region around 90km. We therefore agree with the referee and adjusted the manuscript in Line 137-144 and in the conclusion in Line 367-369.

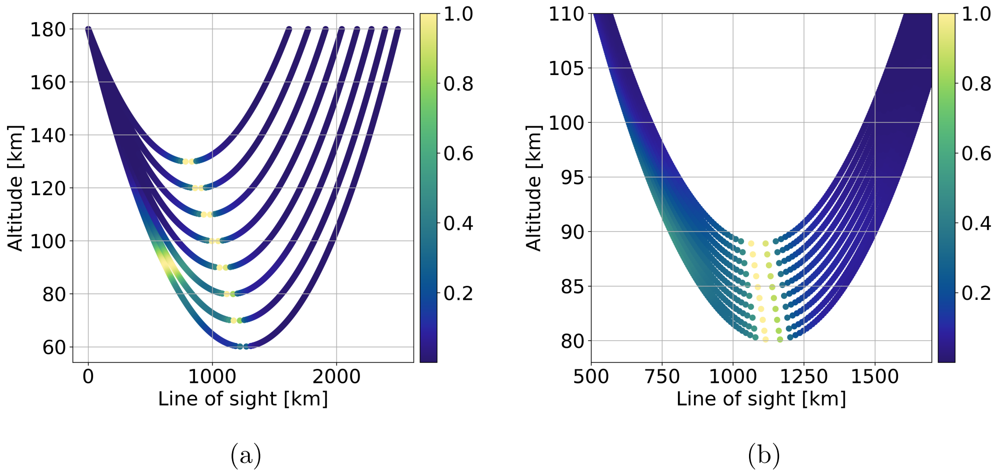
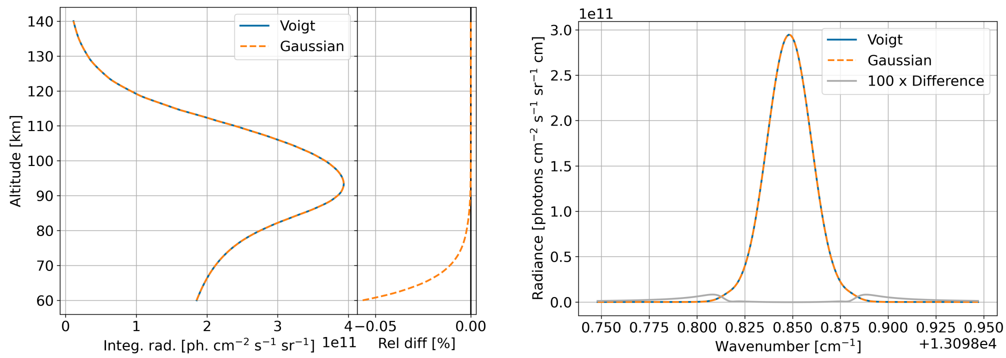


Figure 1: Radiance distribution along the line of sight (LOS) normalized to the maximum of each LOS for (a) selected LOS over the full atmospheric vertical grid from 60km to 180km and (b) a zoom in of the tangent altitudes between 80km and 90km;

It is not enough to simply say you're "using HITRAN" to forward model the line intensities. At line 147, you say that you convolve the line strengths with the ILS. You've skipped a few steps here. How are you accounting for broadening? What types of broadening are you accounting for? Are you actually just convolving the line strengths? Because you need to convolve the emission spectrum (which you calculate from the line strengths), see Babcock and Herzberg, 1948 (doi:10.1086/145062).

The forward model has been tested for Doppler broadening referring to a Gauss shape and Doppler and pressure broadening referring to a Voigt shape. The results are depicted in Figure 2. The spectrally integrated radiance is examined in Figure 2a. It is observed that the simulation using the Gaussian line shape exhibits slight deviations for tangent altitudes below 80km. Nevertheless, these deviations are extremely small and can be neglected. The slightly enhanced flanks of the Voigt line shape, attributed to the pressure-induced Lorentzian shape, become apparent only when the differences are amplified, as demonstrated in Figure 2b. Thus, only Doppler broadening is considered in the forward model.



(a) (b)

Figure 2: (a) Spectrally integrated radiance using Voigt and Gaussian line shape; right panel shows the difference of the simulation using a Gaussian line relative to the simulation using Voigt line shape; (b) strongest emission line for tangent altitude 60km using the Voigt line shape compared to the same emission line in the simulation using Gaussian line shape; the difference (Voigt - Gaussian) is amplified by a factor of 100;

Regarding the convolution of the atmospheric spectrum with the instrument line shape (ILS), it should be noted that the emission lines are extremely narrow compared to the ILS width as shown in Figure 3, and thus can be approximated by a Dirac impulse. The convolution of a function with a Dirac impulse is the function itself and thus, the ILS can be positioned at the position of the emission line and scaled by the line strength. Figure 3 shows that the two methods show only small differences and retrieve the same temperature, where the line strength method is used in the forward model for both cases. Furthermore in this study, the interferogram is built from the line strength it self, as shown in Eq.(1). Thus the forward calculation and the retrieval is consistent in itself. Some discussion is added in Line 157-162.

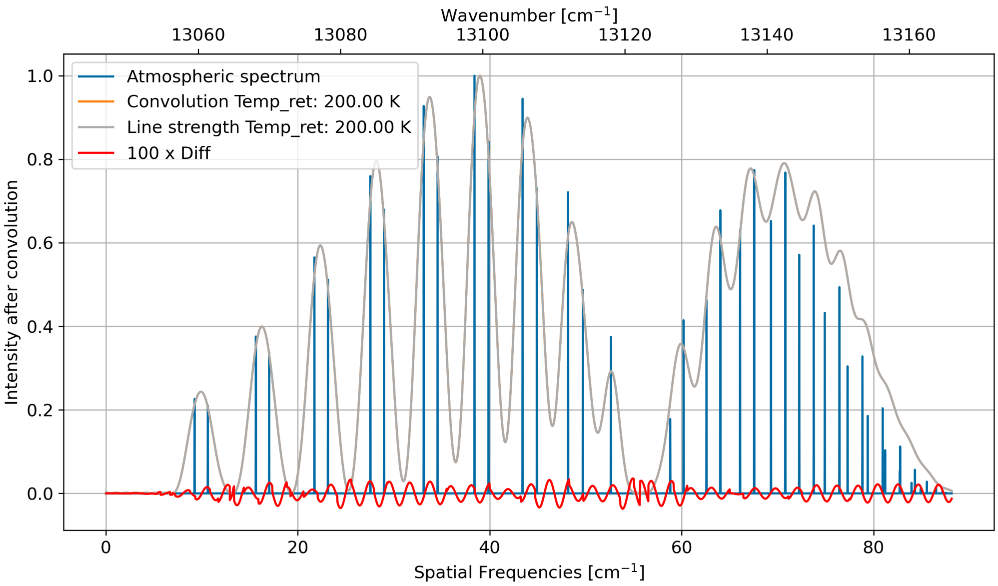


Figure 3: Normalized atmospheric spectrum with resolved narrow emission lines of a homogeneous gas cell for temperature equal to 200K; 'Convolution' refers to the atmospheric spectrum convoluted with the ILS; 'Line strength' refers to the method presented in the paper, where the ILS of each emission line is scaled with the line strength; Temperature indicated in the labels are the retrieved temperature using line strength method in both cases;

The discussion of temperature precision is good. However, a discussion on accuracy is also needed, especially for the daytime retrievals. Specifically, on how you're going to deal with background solar radiation and stray light, and how those will affect the accuracy of the temperature retrievals. It's only at altitudes very close to 90 km where the background solar signal is somewhat negligible compared to the airglow signal. And, if this is intended to be on a nanosat, you're likely going to have limitations on the size of baffle you can use, which means stray light will certainly be an issue. The source of that stray light will be from the bright Earth below, which will have a complicated self-absorption A-band signal, ie, it's not a simple linear function across the spectrum that you need to subtract. These background signals need to be accounted for and discussed.

We agree with the referee that the day-time observations are affected by direct solar radiation and stray light. First sensitivity studies were conducted recently, which showed that the baffle is long enough to neglect direct solar radiation if the sun is not in or very close to the field of view. Stray light due to upwelling radiation specifically from the ground however, affects largely the lower and upper tangent altitudes. Further investigations on this topic and possible correction methods need to be developed for an accurate temperature estimation. This however will not be included in this study. This study mainly focuses on the retrieval of horizontal temperature variations. A small discussion on this is added in Line 150-153.

Specific issues:

Introduction: there have been two instruments launched recently that also use the A-band to measure MLT temperatures, MIGHTI on ICON, and the Swedish MATS satellite instrument. Please mention/reference these as well.

We considered the referee's suggestion and added some information of MIGHTI and MATS instrument in Line 30-37.

Line 26: This sentence is quite vague, please elaborate on why/where/how the instrument was developed.

We elaborated more on the development process of the instrument in Line 37-41.

Lines 26-28: This section is somewhat misleading. It sounds like you're saying that the first instrument (described in Kauffman et al. 2018) was successful in measuring temperature profiles. In that paper, it says that the instrument worked nominally on a rocket launch, however, wasn't able to produce temperature profiles. And the second part of this section makes it sound like a second instrument has been built and is ready to be tested. Is this the case? It should be made clear that Chen et al. 2022 is a simulation study.

We restructured the section in Line 37-41 and Line 45-49 to address this comment in accordance with the previous comment. Furthermore, it is made clear that Chen et al. 2022 is a simulation study in Line 60-61.

Line 55 (and throughout text): "asses" should be "assess"

We corrected the spelling in Line 70, 71, 202 and 336.

Figure 2: It would be helpful to split these into solar max and solar min in different plots. Also, maybe separate daytime and nighttime

We welcome the suggestion of the referee and split the presented 1-D temperature profile into solar minimum and solar maximum condition. Furthermore, the production mechanisms and the expected intensity count per pixel are split into day- and night-time simulations, respectively in Fig.2b,e and 2c,f. Also, the colors has been changed of Fig.2b to address referee #1. Furthermore in Fig.6, the temperature uncertainty is presented individually for day-time and night-time simulations as well, to be consistent. Discussions, captions and references of the figures are adjusted accordingly.

Fig. 10b: the legend should also include the grey interferogram with no gradient

Fig. 10b and its associated caption has been updated, to increase its comprehensibility.

Line 316: I don't recall any special attention being given to results above 120 km. Is this the intended altitude?

120km was the upper limit of the vertical field of view of previous publications (Chen et al., 2022; Kaufmann et al., 2018). This is the first simulation study, which explores the upper limit mainly during day-time conditions, as the lower part of the field of view is affected by self-absorption and stray light from the ground. However, we agree with the referee that the formulation can be misleading. We therefore reformulated the sentence in Line 359-360.

References

Q. Chen, K. Ntokas, B. Linder, L. Krasauskas, M. Ern, P. Preusse, J. Ungermann, E. Becker, M. Kaufmann, and M. Riese. Satellite observations of gravity wave momentum flux in the mesosphere and lower thermosphere (MLT): feasibility and requirements. *Atmospheric Measurement Techniques*, 15(23):7071–7103, Dec. 2022. ISSN 1867-8548. doi: 10.5194/amt-15-7071-2022. URL `https://amt.copernicus.org/articles/15/7071/2022/`.

M. Kaufmann, F. Olschewski, K. Mantel, B. Solheim, G. Shepherd, M. Deiml, J. Liu, R. Song, Q. Chen, O. Wroblowski, D. Wei, Y. Zhu, F. Wagner, F. Loosen, D. Froehlich, T. Neubert, H. Rongen, P. Knieling, P. Toumpas, J. Shan, G. Tang, R. Koppmann, and M. Riese. A highly miniaturized satellite payload based on a spatial heterodyne spectrometer for atmospheric temperature measurements in the mesosphere and lower thermosphere. *Atmospheric Measurement Techniques*, 11(7):3861–3870, July 2018. ISSN 1867-8548. doi: 10.5194/amt-11-3861-2018. URL `https://amt.copernicus.org/articles/11/3861/2018/`.