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Atmospheric Measurement Techniques Discussions

Interactive comment on "Performance Evaluation of THz Atmospheric Limb Sounder (TALIS) of China" *by* Wenyu Wang et al.

Anonymous Referee #2

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In the paper "Performance Evaluation of THz Atmospheric Limb Sounder (TALIS) of China", Wang et al. propose a millimeter/sub-millimeter limb sounder for middle atmosphere observations. Using similar spectral ranges than those chosen for AURA/MLS, this instrument could be one of the few instruments operating after 2020, and, hence, it could be an essential contribution for the monitoring of long term changes of the middle atmosphere. The paper is clearly written and the study well explained (there are minor English issues that can be fixed during the edition). The previous missions and studies are properly acknowledged. I believe the manuscript should be published in AMT but not as it is.

My main concern is that I don't see a plan to really realize this instrument, and the study itself is not enough for a publication. As the authors stated, the concept is not

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Discussion paper

new (similar to Aura/MLS) and only a preliminary estimation of the measurement performance is shown. So I will recommend the authors to improve the manuscript with more information on their plan to realize the mission and on the observation strategy. Also, the instrument field of view and the related parameters (antenna size and scan velocity) are not given in the manuscript. There are key parameters for the assessment of limb sounding performance. Moreover, the retrieval vertical resolution that can be derived from the width of the averaging kernel, should be included in the discussion of the measurement error. The vertical resolution is indeed an essential characteristic of the retrieval performance.

Here below I provide ideas that, I think, could improve the paper.

1) I understand that the realization of such mission is uncertain, and a lot of details are not decided yet. However, it would be nice to know how the authors plan to realize it: When and to who this mission could be proposed? Launch date? Which technology will be used and is it tested with ground based systems? Where the value of Tsys come from? ...

2) What is the size of the antenna? The retrieval vertical resolution should be included in the description of the retrieval performance. The measurement performance of the main products (temperature, H2O, O3) should be compared with those of Aura/MLS.

3) The spectral resolution is lower than MLS at the center of key lines such as O2 @119 GHz and H2O@183 GHz (0.2 MHz vs 2 MHz). This choice has a significant impact on the retrievals in the mesosphere. It should be discussed.

4) More details on the observation strategy could be provided (latitude coverage, local time, . . .). They are improtant when discussing the measurement of radical such as ClO, NO2, NO, HOCl, HO2 which have strong diurnal variations (see for instance Khosravi et al.: Diurnal variation of stratospheric and lower mesospheric HOCl, ClO and HO2 at the equator: comparison of 1-D model calculations with measurements by satellite instruments, Atmos. Chem. Phys., https://doi.org/10.5194/acp-13-7587-2013,

2013.)

Specific comments:

Page 1, L25: “Terahertz limb . . .”, Most of the statements in this sentence are specific of limb sounding in general. I would rather write “Limb sounders . . . in a wide altitude range. In the terahertz domain, the measurement performances are independent of the day-night cycle.”

Page 1, L29: The reference “Ochiai et al., 2017” may not be the best one here since it describes a proposed mission (as indicated further in the paper). The potential for wind measurement has been demonstrated in the mesosphere with Aura/MLS (Wu et al., 2008) and in the stratosphere with JEM/SMILES (Baron et al., 2013).

[Baron et al.: Observation of horizontal winds in the middle-atmosphere between 30S and 55N during the northern winter 2009-2010, Atmos. Chem. Phys., 13, 6049–6064, https://doi.org/10.5194/acp-13-6049-2013, 2013] [Wu et al., Mesospheric doppler wind measurements from Aura Microwave Limb Sounder, Adv. Space Res., 42, 2008]

TALIS should be able to provide wind information between 40 and 60 km (low Tsys and more than 10 strong lines). Are the authors plan to investigate the line-of-sight wind retrievals? If yes, this point could be added in the conclusion as future studies.

Page 2, L15 and 20: To my knowledge TLS is not a selected mission yet.

Page 3, Sect 2.1: What is the antenna size? What is the scan velocity?

Page 3, L18. The O2 line will be measured with less bandwidth than Aura/MLS which is equipped filter bank at -/+ 4 GHz from the O2 line center. This difference should be discussed as well as the consequences for temperature and pressure measurement in the upper-troposphere. Is the O2 line at 239 GHz can compensate the loss of information compared to Aura/MLS?

Page 4, L21 “Using . . . in the 643 GHz bands can measure. . .” should be “Using . . . in the 643 GHz band, one can measure. . ..”

Page 4, L24 “Sato et al., 2013” should be “Sato et al., 2012”.

Page 5, L5: “. . . because of the negative bias in ClO . . .” I find this statement unclear.

Page 5, Fig. 1-4: Stronger colors should be used. The linewidth could also be increased.

Page 7, Eq 1: This equation is not used in the study. I would remove it and discuss Eq 4 instead. Same for the sentence “The so-call phase function . . .” in P8L7.

Page 8, L6: The sentence “Thus the radiance can be converted . . .“ is unclear. The so-call Brightness-temperature unit is obtained from the linear transformation of the intensity in SI using the Rayleigh-Jeans factor: see Eq 3 in Urban et al., 2004 (reference given in P8, L4).

Page 9,‘ L7: “The predicted radiance y_hat are compared . . . ”. I disagree with this statement. The measurement is compared with a noiseless prediction as written in Eq9: chi2 depends on “y-F(x,b)” and not on “y-y_hat”. This part should be rephrased.

Page 9, L15: The notation for the system temperature should be consistent with that used in Tab. 1 (Trec vs Tsys). More information/references on how the Tsys values are defined should be provided.

Page 9, L25: What the authors mean by “strict approach”?

Page 10, L22. Is "vertical separation" the spacing between retrieval altitudes? What is the instrument field-of-view? What is the altitude range between 2 spectra? Are they the same as in the Livesey and Snyder study?

Page 10, L30: The sentence “The true profiles are perturbed to be the a priori profiles” is not clear. Are the true profiles derived by perturbing a priori profiles?

Page 11, L1: I don’t understand the factor sqrt(2). Should it be a factor of 2? Tsys for

SSB = 2 * Tsys for DSB with Eq6 definition (the sideband signals are divided by 2).

In Table 1, it should be clearly indicated that Tsys is for SSB. In Eq 10, it should be clearly indicated that there is a factor to be included for DSB case.

Page 11, L7: "…plotted in the figures." The figure numbers should be indicated "… plotted in Figs 5 to 21". The plots will be clearer if a grid would be added. Also a log scale should be used for the errors (difficult to see details if the errors are small).

Page 11, L15-19: The vertical resolution derived from the width of the averaging kernel should be described and discussed. This is a general comment that should be applied to all products. Also the results should be compared with those from Aura/MLS, at least for the most important ones (temperature, H2O and O3).

Page 11, L19: The sentence "Results of 643 GHz seems not very good" should be rephrased. For me the results look fine in the upper troposphere compared to the two other bands. The error is 2 K, like that for the 240 GHz band and the vertical resolution looks similar.

I think the 2 following information should be given: - Is the tangent height pressure retrieved? - The Zeeman effect polarizes and changes the shape of the O2 lines (Schwartz et al., 2006). Future studies will investigate the impacts in the measurement performance above 60 km. [Schwartz et al., EOS MLS forward model polarized radiative transfer for Zeeman-split oxygen lines, IEEE Transactions on Geoscience and Remote Sensing, doi:10.1109/TGRS.2005.862267]

Page 13, L3: I would rephrase the first sentence as "The H2O profile, another key parameter, can be measured …."

Page 15, L2: "covering" should be "covers"

Page 15, L4: "The other two retrievals … poor precision", I think this sentence is too negative since a good performance is achieved between 20 and 60 km with the 190 and 650 GHz bands.

Page 17, L1: “widely exist” could be removed (-> HNO_3 is a common species in the stratosphere . . ..)

Page 17, L1: It is unclear what “stable lines” mean.

Page 17, L17: The statement “seems bad” is negative and not quantitative. I think it should be stated that the 643 GHz signal is stronger than that in the 240 GHz band, but it is strongly absorbed by O_3 below about 30 km. However after averaging the measurements, information can be retrieved between 25 and 70 km with a precision better than 50%.

Page 19, L3: “can be retrieved by 190 GHz . . .” should be “can be retrieved from the band at 190 GHz . . .. while the band at 643 GHz . . .”

Page 21, L1-2: The statement “is not desirable” is not appropriate. This line will be “desirable” if the radiometer at 640 GHz is not working. Instead, I would state that the best retrievals are perform from the band at 640 GHz but information can also be retrieved from the radiometer at 190 GHz with less precision.”

Page 23, Plot b: What happens at 32 and 65 km? There are strong increases of sensitivity.

Page 25, L4: “exist” should be removed (-> “There are several weak lines in the spectral. . .”)

Page 28-29, The discussion for NO_2 and NO should include the effects of their diurnal variation (this is also true for HO_2, ClO). Around 40 km, NO_2 vanishes in daytime and NO in nighttime. Since the apriori error is assumed proportional to the VMR, no good measurement sensitivity near 40 km can be found for NO_2 in daytime and for NO nighttime. Therefore, I think it is important to clearly define which conditions are considered in the calculation and to discuss the consequences in term of retrieval sensitivity.

Page 33: The expression such as “... temperature < 2 K” should be corrected (e.g., ...

temperature with a precision < 2 K). Several similar cases occur in the conclusion.

Page 33, L4: Any plan to derive IWC? If yes, this could be indicated as future works.

Page 33, L17 The sentence “Although the single scan precision seems not very”, as I already mentioned before, such statement is not appropriate. I think a statement such as “The best sensitivity is found between 70âĂŤ90 km where the precision is better than . . .” would be better.

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