Response to Reviewer Comments

I would like to start this comment by mentioning the fact that I had declined the first invitation of the editor to review this paper with the motivation that I am not an expert in spectroscopy in general, nor in the Zeeman splitting effect and its implications. However, because this paper is pending revision since quite some time, and somehow failed to find enough reviewers, I am moved to accept the challenge on a second invitation by the editor and try to contribute to the subject. Please consider that some of my comments may be affected by a certain degree of naivety. I hope this opening statement will at least encourage some colleagues to put additional effort into committing to their fair share of review duties.

We sincerely thank the reviewer for accepting the review invitation on the second attempt and for providing such detailed and constructive comments. Your efforts have substantially improved the quality and scientific rigor of our manuscript. We particularly appreciate the reviewer's willingness to engage with a topic outside their primary expertise, and their comments have been invaluable in strengthening the manuscript.

General Comment:

This paper explores the effect of Earth's magnetic field on the microwave absorption spectrum around the 60 GHz Oxygen band and the related Zeeman line splitting. It is argued that the proper inclusion of such effects in the RS-LBL model enables the accurate simulation of hyperspectral Brightness Temperature measurement and thus allows to envision the employment of such measurements for the remote sensing of near-space (>20km) atmospheric properties. Unfortunately, I do not consider the study to be of sufficient quality for a publication on the AMT journal.

Throughout the paper, it is implied that the inclusion of the Larsson (2019) Zeeman splitting coefficients in the RS-LBL model is one of the contributions offered by the study. However, after analyzing the RS-LBL code (and its previous versions) it appears to me that this innovation was already implemented in RS-LBL since 2019. Could the authors clarify this point or correct me if I am wrong? Did they contribute to the update? In case RS-LBL was already available I suggest the authors edit the manuscript in a way that does not suggest an original contribution.

Response:

We greatly appreciate the reviewer's careful examination of this point. The reviewer is correct that the RS-LBL model (maintained and updated by Prof. Rosenkranz) has incorporated the Larsson (2019) Zeeman coefficients since the 2024 version. We did not contribute to the RS-LBL model update itself, and we appreciate the reviewer's careful verification of this point.

We have revised the manuscript to avoid any implication that the inclusion of Larsson (2019) coefficients in RS-LBL is our original contribution. The specific contributions of this study are:

(1) **Systematic comparison**: We provide the first systematic comparison of simulation differences between the Hund's case (b) and Larsson (2019) implementations across multiple oxygen absorption lines (7^+, 9^+, 1^-, 13^-, 15^+, 17^+) under varying magnetic field conditions, quantifying the impact of the updated coefficients.

(2) **Detection altitude analysis**: We conduct a comprehensive three-parameter cyclic analysis (B_e, θ, frequency shift) to characterize the detection altitude of oxygen lines under realistic geomagnetic conditions, providing theoretical support for channel design.
(3) **HMAS channel design**: Based on the updated model, we propose the HMAS channel configuration and validate it through weighting function comparisons, brightness temperature simulations, and temperature profile retrieval experiments.

The second major contribution offered by the study is the design of a hyperspectral microwave sounder (HMAS) that would enable "super high resolution". It is not clear to me what is intended with this. The ideal HMAS instrument is compared with the existing SSMIS only in terms of weighting function density which is hardly indicative of effective benefits of remote sensing instrument designs. Similar papers (like Aires 2015 from the list of references) developed also ideal instruments but supported their position with an extensive analysis of error sources, effects of noise and channel resolution and information content distributions. All of this is missing from the presented study and makes this section not acceptable according to the AMT standards.

Response:
We fully agree with the reviewer's assessment that the original manuscript lacked sufficient quantitative validation. To address this, we have added a new Section 4.4 presenting temperature profile retrieval experiments “Comparison of SSMIS and HMAS Temperature Profile Retrieval Results”, which includes:
(1) Temperature profile retrieval experiments using an improved back propagation (BP) neural network with dual residual blocks and squeeze-and-excitation (SE) attention mechanism, based on 144 days of simulated brightness temperature data from both HMAS and SSMIS channels.
(2) Quantitative comparison metrics: RMSE, MAE, R^2, correlation coefficient, and MAPE are reported for both instruments, with HMAS achieving an RMSE of 7.652±0.389 K compared to 7.858±0.314 K for SSMIS.
(3) Daily mean statistics from January to December 2024, demonstrating that HMAS consistently outperforms SSMIS across all evaluation metrics.
(4) Discussion of uncertainties: We have added discussion on the sources of uncertainty in the channel design, including the effects of instrument noise, channel bandwidth selection, and the impact of the “double-peaked” weighting function in linear polarization.

We believe these additions bring the manuscript to a standard consistent with AMT requirements.

I believe that the authors had a good idea exploring the effect of more accurate simulations due to the updated Zeeman coefficients but failed to construct a solid scientific approach to the idealization of HMAS. One possible approach would have been to try to replicate the study of Aires 2015 with the updated RS-LBL; analyzing the effect of varying magnetic field would allow for very interesting and novel insights. The authors decided to propose an original instrument, but I argue that in this case proper analysis of the potential sources of uncertainty is still fundamental for the presentation of a remote sensing concept. Basic analysis of retrieval performances and/or contribution to data assimilation are also necessary.

Issues:
I had quite a hard time understanding the text in subsection 2.1 and honestly, I had to refer to other sources to fill the gaps. Part of my difficulties arise from the fact that the text is unnecessarily convoluted (e.g Line

69-70 "The magnetic quantum number describes the quantum number..."), uncommon concepts like "fine-structure Hamiltonians" are assumed to be common jargon and symbols and quantities are introduced and never used (vic. sigma, deltaM, deltaJ deltaW and so on). Indeed, the authors demonstrate a very solid knowledge of the topic discussed, but the audience would benefit from a more concise and practical explanation of the advantages introduced by the Larsson 2019 update, which I believe would tend to meld the subsection 2.1 with the later section 3. Regardless of the solution the authors will implement, the problem is that subsection 2.1, as it is, is not useful.

Response:

We sincerely thank the reviewer for this candid and constructive feedback, which prompted a substantial reorganization of this subsection. In the revised manuscript, we made the following improvements:

(1) **Removed unused symbols and clarified the physical description**: The convoluted description of the magnetic quantum number and the introduction of symbols such as ΔJ, ΔM, π component, and $\sigma\pm$ component, which are introduced but not subsequently used in the manuscript, have all been removed. The revised subsection now provides a concise description of the Zeeman splitting effect, focusing only on the core concepts (g-factor, energy perturbation) that are directly used in the subsequent analysis.

(2) **Simplified the transition from Hund's case (b) to Larsson (2019)**: The original manuscripts included the vague reference "Please refer to (Larsson et al., 2019)" without explaining what the reader should find there. This has been replaced with a clear statement that the derivation and calculated values for individual N and J states are provided by Larsson et al. (2019), and the physical significance of the update is explicitly stated: Eq. (3) captures all coupling contributions from electron spin, anisotropic spin, and molecular rotation, substantially improving the accuracy for low-energy oxygen lines.

(3) **Strengthened the connection between theory and application**: We have added two key sentences at the end of subsection 2.1 that directly link the theoretical discussion to the quantitative results in Section 3: (i) "Consequently, it introduces systematic biases for oxygen absorption lines with low rotational quantum numbers (J_N < 10), which are precisely the lines most sensitive to upper atmospheric temperature." (ii) "As demonstrated by Dong et al. (2026), the updated coefficients effectively reduce simulation biases in upper atmospheric sounding channels, a critical improvement for near space temperature profiling." These additions help the reader understand why the theoretical details matter for the practical application of near space temperature profiling.

Point 1:

Line 101, Delete "Please refer to (Larsson et al., 2019)" or specify for what we need to investigate the source that is not covered in the paragraph.

Responses:

We have deleted the phrase "Please refer to (Larsson et al., 2019)" as suggested. Larsson et al. (2019) is now cited only as the source reference for the updated Zeeman splitting coefficients.

Point 2:

Line 111, Delete "In mathematics"

Responses:

We have deleted as suggested.

Point 3:

Figure 1 - please put measuring units on y axis (K)

Response:

Thanks! The y-axis unit (K) are added to Figure 1.

Point 4:

Line 406, why does the sentence begin with "since"?

Response:

We thank the reviewer for pointing this out. we revised the concluding paragraph: “This study focuses on the HMAS 150 channels at 60-63 GHz, while additional channel configurations at other frequency bands remain to be investigated. The current HMAS channel configuration is a conservative adaptation of existing techniques. In the future, channel setups can be fully adjusted based on technological capabilities, though all configurations require detailed simulation and analysis of the O2 fine-structure lines. The temperature profile retrieval framework developed in this study can also be applied to compare the performance of HMAS, MAS, and SSMIS channel configurations.”

Point 5:

Line 409, why does this sentence mention temperature retrievals if there are no retrievals presented in the paper?

Response:

Thank you for pointing this out. We have now added a complete temperature profile retrieval experiment in Section 4.4.

Point 6:

The LBL code is mentioned in the Code Availability section, however this section should be devoted to the permanent links (I can recommend Zenodo) to the code (and data) necessary to ensure the reproducibility of the results. At the same time the LBL should be cited in the text as suggested by its authors:: How to cite: Rosenkranz, P.W.: Line-by-line microwave radiative transfer (non-scattering) [software] (version YYYY/MM/DD), http://cetemps.aquila. infn.it/mwrnet/lblmrt_ns.html (last access: DD Month YYYY).

Response:

We thank the reviewer for this important suggestion. In the Introduction, we cite the RS-LBL code following the format recommended by its author: Rosenkranz, P.W.: Line-by-line microwave radiative transfer (non-scattering) [software] (version 2024), https://cetemps.aquila.infn.it/mwrnet/lblmrt_ns.html (last access: 18 May 2025).

We sincerely thank the reviewer for these detailed and insightful comments, which have significantly improved the scientific rigor and clarity of our manuscript.