Responses to Reviewers' Comments

We are sincerely grateful to the editor and reviewers for their valuable time spent on reviewing our manuscript. The comments are constructive and valuable, and we have tried our best to address the issues raised by the reviewers in revised manuscript. Please find our item-by-item response (in bold) to the comments (in black) raised by the reviewers, and the paragraphs added in the revised manuscript (in red).

Reviewer #2

General Comments: This manuscript presents a timely and valuable contribution to the field of satellite remote sensing of thermodynamic profiles of the atmosphere, focusing on the upcoming FY-4M geostationary microwave satellite. The authors propose a complete retrieval algorithm with an information-content-based channel optimization scheme, specifically designed for all-weather atmospheric temperature profiling over the ocean. The work is methodologically sound, well-structured, and addresses a critical operational challenge, balancing retrieval accuracy with computational efficiency for a sensor with over 100 channels. The novelty lies in the comprehensive analysis of the FY-4M's unique hyperspectral temperature channels and several high-frequency channels, and in the comparative assessment of their performance under clear, cloudy, and precipitating conditions. The results are promising, showing that the optimized AC95 channel configuration can achieve a favorable balance. I recommend the manuscript for publication after the authors address the following comments and suggestions.

Reply: We thank the reviewer for the encouragement and the valuable comments to improve our manuscript. All the comments have been addressed in the revised manuscript.

Specific Comments:

Q1: The sequential forward selection method in Sec. 2.3 is clearly described. However, the process of treating all hyperspectral channels as a superchannel (Section 3) is a simplification that merits more discussion. While it demonstrates the overall importance of this band, it does not provide a ranking of individual hyperspectral channels. This is a minor limitation, as the operational algorithm (AC95) includes the entire band. I suggest the authors briefly acknowledge this point in the discussion or conclusions, noting that future work might explore internal optimization within the hyperspectral set.

Reply: We appreciate this constructive suggestion. We agree that treating all hyperspectral channels as a superchannel is a simplified approach. Although this treatment effectively demonstrates the irreplaceable value of this band for

temperature sounding, it does not provide an independent ranking of the individual hyperspectral channels. We have changed the 'Conclusion' section to 'Conclusions and Discussion' and added the following statement:

Also, it should be noted that treating the hyperspectral band as a single superchannel effectively demonstrates the irreplaceable value of this band for temperature sounding, but the differences in information contribution among individual hyperspectral channels haven't been further quantified here. Future work will focus on channel selection within the hyperspectral band, with the aim of identifying the most critical sounding channels without substantially degrading retrieval accuracy.

Q2: Treatment of observation error (sec. 2.3 & 2.4) needs some discussion. The authors state that $\boldsymbol{R_{atm}}$ and $\boldsymbol{R_{surf}}$ are diagonal matrices, yet the final $\boldsymbol{R_f}$ is constructed using a Gaussian correlation matrix with a 1 GHz correlation length. This creates confusion for the reader. Are the forward model errors assumed to be correlated or uncorrelated? I suspect the authors' actual intent is that the diagonal matrices are intermediate constructs used solely to store the estimated error variances for each channel, while the 1 GHz correlation matrix is subsequently applied to impose spectral error correlations, yielding the final full covariance matrix $\boldsymbol{R_f}$. If this is the case, the manuscript would benefit from explicitly describing this two-step construction process to resolve the apparent contradiction. In addition, please clarify the basis for selecting 1 GHz as the correlation length. Is this value physically motivated (e.g., based on the spectral spacing of the channels or the oxygen absorption line width), or is it adopted from previous studies? A brief justification or reference would suffice.

Reply: We fully agree with the reviewer that the treatment of observation error should be clarified. According to the method by Aires et al. (2015), the two-step construction process of the $\boldsymbol{R_f}$ matrix and the basis for selecting 1 GHz as the correlation length have been clarified in the revised manuscript:

The radiative transfer error covariance matrix $\boldsymbol{R_f}$ was constructed in two steps (Aires et al., 2015). First, the atmospheric and surface modeling error variances were estimated separately for each channel and stored as diagonal matrices, $\boldsymbol{R_{atm}}$ and $\boldsymbol{R_{surf}}$, respectively. $\boldsymbol{R_{atm}}$ is assumed to increase linearly with frequency and $\boldsymbol{R_{surf}}$ is defined as $\Delta \mathrm{e} \boldsymbol{T_s} \boldsymbol{\tau}^2$, where $\Delta \mathrm{e} = 0.01$ is the surface emissivity error, $\boldsymbol{T_s}$ is the surface temperature, and $\boldsymbol{\tau}$ is the atmospheric transmittance. Second, to represent the spectral correlations of these errors, a Gaussian function truncated at ± 2 standard deviations, with a correlation length of 1 GHz, was applied to these variance estimates to construct the final non-diagonal matrix $\boldsymbol{R_f}$.

Aires, F., Prigent, C., Orlandi, E., Milz, M., Eriksson, P., Crewell, S., Lin, C.-C., and Kangas, V.: Microwave hyperspectral measurements for temperature and humidity atmospheric profiling from satellite: the clear-sky case, J. Geophys. Res. Atmos., 120, 11334-11351, https://doi.org/10.1002/2015JD023331, 2015.

Q3: The retrieval RMSE for the AC95 scheme during precipitation is reported as within 0.8 K, which is a very good result. However, the text mentions that retrieval errors increase noticeably due to complex scattering. The Bayesian algorithm relies on a comprehensive a priori database that includes simulated brightness temperatures from a plane-parallel MWRT model. This model has known limitations in representing 3D scattering effects. How might these model limitations affect the generalizability of the retrieval to more intense or convective precipitation systems that are not well represented in the training database? A short note on this would be valuable for the reader.

Reply: We appreciate this suggestion. We agree that the plane-parallel MWRT model could not fully capture the complexity and variability of precipitating scenes because of its limitations in representing 3D scattering effects, which may reduce retrieval accuracy. In response to a related comment from another reviewer, we conducted a simulation-based retrieval experiment for Typhoon Doksuri and added the results to the "Results" section. The results show that the main temperature structures of the typhoon were reasonably reproduced. Except for the NHC90 scheme, the retrieval biases were generally close to 0 K, with RMSE values below 1 K at most vertical levels. These results support the potential applicability of the proposed algorithm under simulated strong convection conditions. Nevertheless, this simulation-based experiment does not eliminate the limitation identified by the reviewer. As this issue is also closely related to another reviewer's request, we have added the following paragraph to the 'Results' section as follows:

Liu et al. (2026) reported temperature RMSEs of 3.07, 3.56, and 3.26 K for the FengYun-3E (FY-3E) vertical atmospheric sounding system (VASS) product under clear-sky, cloudy, and all-sky conditions, respectively, based on validation against radiosonde observations. The lower RMSEs obtained in the present study demonstrate the potential of the proposed algorithm. Considering the used plane-parallel MWRT model cannot fully represent three-dimensional scattering effects, however, the retrieval errors for real FY-4M observations may be larger than those indicated by the present simulation experiments. Therefore, the actual retrieval performance should be further evaluated using real on-orbit FY-4M observations in the future.

Liu, S., Xu, J., Chen, L., Liu, H., Wang, W., He, J., Zhu, S., and Shi, J.: Comprehensive validation of temperature and humidity profile retrievals from FengYun-3E, IEEE Trans. Geosci. Remote Sens., 64, 4102214, https://doi.org/10.1109/TGRS.2026.3658572, 2026.

Q4: The 14% reduction in computational time for AC95 compared to the full AC configuration is a key finding. However, it is not entirely clear whether the training phase or the retrieval phase is the main cost driver. In operational scenarios, the retrieval is the time-critical step. Could the authors clarify which part of the algorithm benefits most from the channel reduction? This would help assess the real-world operational advantage.

Reply: Thank you for your valuable comment. The 14% reduction in computational time reported in this study mainly comes from the retrieval phase, rather than the training phase, namely the construction of the a priori database. The training phase is an offline process that only needs to be performed once, whereas the retrieval process must be repeatedly performed for each observed pixel. Its computational cost is therefore directly affected by the number of channels used and represents the main computational burden in operational applications. By reducing the number of channels involved in brightness temperature matching and probability calculation, the AC95 scheme reduces the computational cost of each retrieval. We have added the following clarification to the corresponding part of the 'Results' section in the revised manuscript:

It should be noted that this reduction in computational time mainly comes from the retrieval phase. The AC95 scheme uses fewer channels in brightness temperature matching and probability calculation, thereby reducing the computational cost of each retrieval while preserving retrieval accuracy.

Minor Technical Corrections:

Q1: The formula of Eq.4 is correct, but it would benefit from a definition of the integration time (t). Is this the dwell time per channel, or the overall integration time? I assume it is the dwell time, but clarifying this in the text would be helpful.

Reply: We appreciate this suggestion. The integration time (t) is the dwell time and we have clarified in the revised manuscript:

Here, ***t*** denotes the dwell time for each channel.

Q2: The caption of Fig. 7 says "Similar to Fig. 6, but including hyperspectral channels..." This could be more explicit. I suggest: "Similar to Fig. 6, but ranking all

FY-4M channels, with the entire hyperspectral band (52.6-57.3 GHz) treated as a single predefined channel with the highest priority.

Reply: Thank you for this helpful suggestion. We have revised the caption of Fig. 7 accordingly to make the treatment of the hyperspectral band more explicit.

Q3: The English is generally fluent and readable. A few sentences are slightly convoluted; a minor stylistic edit by a native English speaker would further improve clarity.

Reply: We appreciate this suggestion. We have carefully proofread and polished the language throughout the manuscript to improve clarity. All revised parts have been marked in red in the revised manuscript.