Section 2.1: I would like to suggest that this section be split into two subsections, one (L 99-103) relating to PMR characteristics, the other to MWRI-RM (L 104-114).

Response：
Thank you for your suggestion. However, considering that this section provides only a brief introduction to each instrument and to maintain narrative coherence, we have decided to keep both instrument descriptions under the "Data Source and Characteristics" section.

L.107: Here the authors correctly cite the paper where a table of MWRI-RM characteristics is reported. However, I would suggest that this table be added to the paper, as MWRI-RM is the instrument on which the paper is based.

Response：
Thank you for your suggestion. In response, we have added a table detailing the specifications of the MWRI-RM channels to the revised manuscript (**Table 1**).

Section 2.2: I think a summary table of the main characteristics of the dataset used in this paper (time period, number of samples, number of precipitating and nonprecipitating samples, number of samples over sea and over land) would make the paper more readable.
Section 2.3: Again, I would suggest that the authors add a table summarising the input datasets of the three experiments reported.

Response：
Thank you for your helpful suggestions. In response, we have added a summary table of the main characteristics of the dataset used in this paper (time period, number of samples, number of precipitating and non-precipitating samples, and the number of samples over sea and land) in the revised manuscript, specifically between **lines 165 and 172.**

L.152: It is not clear to me what the oversampling consists of. Are the same data duplicated in the dataset? I think the authors need to specify this and discuss the consequences of this choice. In particular, how can this operation affect the dataset,
which is split into two sub-datasets for the training and test phases? Also, I suspect that the precipitating samples were duplicated, not the non-precipitating pixels.

Response：
Thank you for your comment. Upon reviewing the manuscript, we realized that there was an error in our original description. We did not oversample non-precipitating samples as stated, but instead downsampled them to match the number of precipitating samples. This approach was taken to balance the dataset and ensure equal representation of both sample types. We appreciate your feedback and have corrected this point in the revised manuscript to avoid further confusion. Thank you for highlighting this issue.

L.155-156: I think the authors need to provide more information on how the "training" and "test" samples were selected. Is this separation based on a random process? Or by selecting observations from a defined time period? This selection plays a very important role in the development of machine learning approaches. If the two sub-datasets are created by randomly selecting the pair samples, the two sub-datasets may be highly correlated, so the statistical scores calculated over the test dataset may be not representative. In this case, the authors must recalculate the statistical indices on a test dataset independent of the training dataset - using time periods or orbits not used in training.

Table 2 and Figure 5: I guess that the statistical scores and the mean profiles are calculated over the test dataset; however, this information is not clearly reported in the text. I would like to suggest that the authors specify which dataset the scores are calculated on. Again, the RMSE, STD, and MBE have been calculated only for the true positive (for precipitating samples) and for the true negative (for non-precipitating samples)? This information must be added to the text.

Response:
Thank you for your insightful comment. To address your concern about potential correlations between training and test datasets, we revised our evaluation to use an independent test dataset collected from a different time period (20231201–20231211) that was not included in the training data.
The statistical scores recalculated on this independent test dataset show results that are similar to our previous evaluations, indicating that the model demonstrates a certain degree of generalization ability. We have updated the manuscript to reflect this change and included the revised evaluation details. Thank you for highlighting this critical point, which has helped us improve the robustness of our analysis.

L.214-218: It is not clear to me how precipitating/non precipitating pixels are defined. Is it related to a reflectivity threshold? Is it the 12 dBZ threshold cited at L. 135?

Response:
Thank you for your comment. To avoid any potential misunderstanding, we have removed the use of the F1 score in the revised manuscript. This decision was made because we do not have a reliable label to definitively classify precipitating and non-precipitating pixels, and the absence of such labels could lead to ambiguities in the evaluation.
We appreciate your feedback, which has helped clarify and refine the presentation of our results.

L.234-235 and Figure 5: how the melting layer height is determined?

Response:
Thank you for your comment. Currently, we do not have access to PMR-specific melting layer products. To provide context for the melting layer height, we referred to the analysis by Hu et al. (2024), which investigates the quasi-global climatological features of the melting layer using data from the Dual-frequency Precipitation Radar onboard the Global Precipitation Measurement Mission Core Observatory (2018–2022).
Their findings indicate that melting layer heights vary with latitude, being higher in tropical regions (30°S–30°N) and lower in mid- and high-latitudes. Specifically, in tropical regions (0°–30°N), the top of the melting layer is generally between 4–5 km, while the bottom height is primarily between 2–4 km. These observations align with the results presented in our study.
To avoid potential controversy, we have removed the yellow shaded area indicating the melting layer from Figure 5 in the revised manuscript. We appreciate your feedback, which has helped improve the clarity and accuracy of our figures and discussion.

L. 258-260: The reported period is outside the period of the development dataset. This makes the results more valid but must be stated in the text.

Response:
Thank you for your comments. Before the official data release, we were granted access to PMR observations from July 2023, including data from Typhoon Khanun and the Beijing extreme rainfall event. These two cases represent extreme precipitation events, making them valuable for further evaluating the model's generalization ability. We have clarified this in the revised manuscript in **Lines 281-283**

Figure 8: A general weakness of the paper is the lack of references to the brightness temperatures observed by the MWRI-RM, which are the input data to the deep learning algorithm. I understand that reporting TB maps can be difficult given the number of channels, but perhaps a TB map could be added to Figure 8 - or, for example, a 3D reconstruction using the TB observed - similar to those for the reflectivity levels in Figure 8 - by the oxygen absorption band channels.

L. 324-325 and Figure 9: Certainly, the comparison between the reconstructed reflectivities and the ground-based radar observations is a very good element that makes the analysis more valid. However, it is not clear to me on which element the statement "The reconstructed precipitation reflectivity distributions based on MWRI-RM observations are consistent with actual ground-based radar observations" is based. A more precise analysis is needed - e.g. some statement about the areas where precipitation is detected, or about the position of the reflectivity maxima, or something similar.

Response:

Thank you for your valuable suggestion. We have revised the manuscript to provide a more precise analysis comparing the reconstructed reflectivities with the ground-based radar observations **(Lines 370-374)**.

Minor Comments

1) L. 15: “Precipitation” instead of “precipitation”
2) Figure 5: I would like to suggest that the authors add the highlighted shading to the center and right panels as well (b, c, e, f).
3) Figure 6: I would suggest that the authors add the section line to the other center panels (b, e, h, k) and a dashed line corresponding to a height of 4 km to the right panels (c, f, i, l). I suggest adding labels to the x axes (latitude values?) of the right panels (c, f, i, l).
4) Figure 7: same as Figure 6.
5) L. 273-309, Figures 6 and 7: In general, I suggest adding lat/lon references to the description of the left and center panels - e. g., L. 282: underestimated reflectivity values in the northwestern rainbands (26 °N, 130 ° E) while overestimating those east of Khanun’s center (23 ° N, 133 ° E) - and height reference to the right panel description - e. g., L. 283 The reconstructed melting layer structure was overly smooth (between 4 and 5 km). The addition of labels to the x axes of the right panels will make this analysis easier.
6) L. 298-324, Figure 8 and Figure 9: It would be useful to add a lat/lon reference linked to the name of the regions - e. g., L. 318: over southern Beijing (-° N, - ° E), central Hebei (-° N, - ° E), and Tianji (-° N, - ° E) - or a label above the map.

Response:

Thank you for your constructive suggestions regarding Figures 6, 7 and 8. We have carefully addressed each of your points. These revisions ensure that the figures are more precise, informative, and easier to interpret, addressing the concerns raised. Thank you for your detailed suggestions, which have significantly improved the clarity of our analysis.