Response to the Review

Thank you for answering the comments of the first 2 reviewers and modifying the manuscript (mostly) accordingly. I asked the opinion of another referee, which comments should also be integrated in a new version of the manuscript.

Furthermore, I am also not completely satisfied with the modifications you made to the manuscript, following upon the comments of the first two reviewers. These are both minor and some larger comments. A non-exhaustive list is provided here (please note that the line numbers refer to the manuscript with tracked changes).

Response: Thank you for your continued time and effort in reviewing our manuscript, and for securing the additional feedback from the third referee. We appreciate your candidness regarding the previous round of modifications. We are carefully reviewing the new comments from the third referee, as well as the list of lingering concerns you provided. We are committed to thoroughly addressing both the minor and major issues raised, and we will revise the manuscript to ensure all modifications fully resolve the outstanding points and meet your expectations. A detailed, point-by-point response addressing both your feedback and the third referee's comments will be submitted alongside the newly revised manuscript.

* line 10: "fundamentally limited" has not been changed to "significantly affected", although you stated in your response that you have done so. This is not correct. Please go through the entire manuscript again and correct them if you stated that you did. In a next revision round, I will go through them one by one.

Response: We sincerely apologize for this oversight. Reviewer is absolutely correct; while we updated the phrasing in the Conclusion (Line 584), we inadvertently missed the instance in the Abstract (Line 10), which still read "fundamentally limited". We have now corrected this specific instance to "significantly affected" in the newly revised manuscript.

* lines 84, 573, 577: another example of an incomplete correction despite stating differently: the acronym EM is still used. It cannot be that hard to search for "EM" in the manuscript and write it out at all locations.

Response: We sincerely apologize for this repeated oversight. Reviewer is entirely correct; we missed the remaining instances of the acronym "EM" during our previous round of edits. We have now explicitly written out "Eastern Mediterranean" at the lines you indicated, and we have conducted a strict text-search throughout the entirety of the manuscript to guarantee that the acronym has been completely eliminated.

* line 123: provide references for this "well-documented limitation"

Response: We have now corrected this specific instance to " well-documented limitation " in the newly revised manuscript by appropriate references.

* in section 2.2.1, as opposed to sections 2.2.2 and 2.2.3, you do not describe the MWR data processing. Here, you should mention the neural network training, the 'supervised linear regression model" developed to calibrate the MWR observations (i.e. move lines 216-224 to section 2.2.1)
Response: We thank the reviewer for highlighting this structural imbalance in the methodology section. We agree that Section 2.2.1 lacked the necessary data processing details compared to the subsequent sections. We have revised Section 2.2.1 to explicitly describe the manufacturer's Neural Network (NN) algorithm used for the native thermodynamic retrievals. Additionally, as requested, we have moved the detailed explanation of the supervised linear regression model directly into this section to ensure all MWR processing steps are consolidated logically in one place.

* line 164: "apples-to-apples" is not a scientific wording
Response: We thank the reviewer for highlighting this colloquialism. We completely agree that the phrasing was too informal for a scientific manuscript. We have removed the idiom and revised the sentence to use precise academic terminology, ensuring the rigorous nature of the intercomparison is properly conveyed.

* line 168: "It is well established" --> provide references to proof this
Response: We thank the reviewer for noting the absence of formal literature support for this statement. It is a standard practice in GNSS and microwave radiometry intercomparisons to cap radiosonde integrations, as the moisture mass above 10 km is thermodynamically restricted and falls well below standard measurement uncertainty. To ensure this claim is rigorously grounded in the text, we have updated the sentence to explicitly cite Van Baelen et al. (2005).

* lines 169-170: you haven't described the Hv yet, so you cannot use this as an argument. You cannot refer forward that much in a manuscript. Drop this sentence.
Response: We thank the reviewer for catching this structural flaw. We completely agree that referencing the scale height (Hv) and its corresponding equation before they are formally introduced disrupts the logical flow of the manuscript. As requested, we have entirely dropped this sentence from Section 2.2.2.

* line 172-173: you cannot compare the value of a (upper-tropospheric) integrated water column with the humidity measurement uncertainty at a specific pressure level. This is not an "apples-to-apples" comparison =:-)
Response: We thank the reviewer for catching this conceptual mismatch (and we appreciate the well-placed callback to our previous wording!). You are completely right; comparing an integrated mass (fractions of a millimeter) directly to the 4% relative uncertainty of an individual point measurement is physically inconsistent and mathematically flawed.
We have revised the sentence to properly compare integrated quantities. The text now accurately argues that the residual integrated water vapour mass above 10 km is so exceptionally small that

it falls well within the absolute uncertainty margins (typically 1–2 mm) of the total-column radiosonde IWV integration itself, rendering the cutoff negligible for total column comparisons.

* lines 239-257: this theoretical framework of the error propagation in the IWV retrieval is of no use if the variables in the equations are not feed with (derived) numbers. This should not been done here, but in Section 3.4, you should explicitly use this theoretical framework and refer to the equations written here, when you feed them with the uncertainty estimates obtained.

Response: We thank the reviewer for this excellent suggestion. We agree that introducing the partial derivative framework in Section 2.3 without explicitly populating its variables with our empirically derived numbers later in the manuscript represented a missed opportunity to ground the theory. We have revised Section 3.4.3 (Uncertainty Budget Analysis) to explicitly refer back to the error propagation equations established in Section 2.3. We now directly feed the empirically derived uncertainties (e.g., our specific pressure errors and Tm RMSE values) into the corresponding theoretical variables to physically quantify the separated error components. This tightly couples our theoretical framework with the observed results.

* lines 261-282: as you might remember, in my first review of your manuscript, I was also not a big fan of the Hv part, as is reviewer 2. But I'm fine with keeping it in the manuscript, as long as your arguments are found back in the manuscript. In the mentioned text part, the Hv concept is introduced out of the blue: the reader really does not have a clue of what Hv will be used for and it really disturbs the flow of the paper. To my opinion, you should move this "concept introduction" to the part of the manuscript where it is really used, i.e. in section 3.2.

Response: We thank the reviewer for this constructive observation regarding the manuscript's structure. We agree that the extensive conceptual introduction of the water vapour scale height (Hv) in Section 2.4 disrupted the logical flow, as its specific diagnostic purpose is much easier to grasp when presented directly alongside the results. To resolve this, we have significantly condensed the text in Section 2.4, retaining only the fundamental mathematical definition (Equation 12) and the essential mathematical boundaries (the 4 km limit and the 0.1–4.0 km quality assurance filter) required for the methodology. The broader conceptual justification—explaining exactly *why* Hv is utilized as a diagnostic metric to quantify the passive sensor's smoothing error—has been relocated to the beginning of Section 3.2. We believe this structural adjustment greatly improves the readability of the paper and ensures the reader understands the utility of Hv at the exact moment it is applied.

* lines 444-445: the Bevis empirical relationship is based on the surface temperature, not the surface skin temperature. Please remove all the skin-level, skin temperature references in the manuscript.

Response: We thank the reviewer for pointing out this important terminological inaccuracy. Reviewer is entirely correct that the Bevis empirical relationship relie on standard surface air temperature, not radiometric skin temperature. We have thoroughly reviewed the manuscript and completely removed all references to "skin temperature" and "skin-level heating." We have

updated the text in both the Results and Discussion sections to accurately refer to standard surface temperature dynamics, ensuring scientific precision throughout the physical explanation.

* lines 471-472: this sentence should have appeared in section 2.2.1. If you want to repeat it here, refer to this section as well. * line 476: exactly the same comment as the previous one: the linear correction model belongs in section 2.2.1. Refer to this section here as well.
Response: We thank the reviewer for pointing out these structural inconsistencies. We entirely agree that the mathematical formulation of the linear correction model and the specific details regarding the dataset splitting strategy (training versus validation sets) fundamentally belong in the methodology section dedicated to the Microwave Radiometer. We have relocated the detailed descriptions of the dataset split and the linear regression equation to Section 2.2.1. In the subsequent sections where these concepts are applied or reiterated, we have streamlined the text and added explicit cross-references back to Section 2.2.1 to maintain a cohesive flow and avoid unnecessary repetition.

* line 497: as the third reviewer was asking for it, the right reference is "Van Malderen, R.; Pottiaux, E.; Stankunavicius, G.; Beirle, S.; Wagner, T.; Brenot, H.; Bruyninx, C.; Jones, J. Global Spatiotemporal Variability of Integrated Water Vapor Derived from GPS, GOME/SCIAMACHY and ERA-Interim: Annual Cycle, Frequency Distribution and Linear Trends. Remote Sens. 2022, 14, 1050. https://doi.org/10.3390/rs14041050."
Response: We thank the reviewer (and the third referee) for catching this critical citation error. discovered that a reference management error caused the wrong title and author list to be associated with this specific publication and DOI in our bibliography. We have completely updated the reference list to accurately reflect the correct paper (*Global Spatiotemporal Variability of Integrated Water Vapor*...). Because the primary author and year remain the same, the in-text citation at line 497 (Van Malderen et al., 2022) remains textually unchanged, but it now properly links to the correct, updated bibliographic entry.