Reviewer#01 Comment:

The work presented in the manuscript address the possibility to improve the accuracy of the conversion from Zenith Wet Daley (ZWD), estimated from observations with a ground-based GNSS station, to Integrated Water Vapour (IWV) by adding data from a microwave radiometer capable of providing information about the temperature profile in the atmosphere. Although these results are of course unique for this specific station, the findings are primarily of interest for users that have (or are considering to buy) this type of radiometer. It is a much larger investment compared to the GNSS ground station itself. In any case, because as far as I know it is a new concept, and it makes sense that the study is made available for the entire scientific community.

My criticism is that when reading the manuscript it gives the impression that it is the uncertainties in the mean temperature that is the limiting factor for the quality of the IWV. I like to point out that even during extreme conditions (regarding the temperature profile) the uncertainty in the final estimate of the IWV, the Zenith Total Delay (ZTD) is at least as important. See e. g. Table 4 in: Ning et al. (2016). The uncertainty of the atmospheric integrated water vapour estimated from GNSS observations, Atmos. Meas. Tech., 9, pp. 79-92, https://doi.org/10.5194/amt-9-79-2016. Below I suggest alternative wordings in order to nuance this issue.

Response: We sincerely thank the reviewer for their encouraging assessment of our work and for recognizing the value of this synergistic concept for the broader scientific community. We also deeply appreciate the reviewer's constructive and highly accurate criticism regarding our initial framing of the error budget.

The reviewer is entirely correct. In our effort to highlight the specific thermodynamic errors associated with static climatological models, the original draft inadvertently gave the impression that the weighted mean temperature (Tm) is the sole or universally dominant limiting factor for Integrated Water Vapor (IWV) quality. We completely agree that the uncertainty originating from the Zenith Total Delay (ZTD) estimation—stemming from mapping functions, satellite orbits, clocks, and site-dependent electromagnetic effects—is at least as important, and frequently exceeds the error introduced by the Tm conversion factor.

We thank the reviewer for directing us to the comprehensive uncertainty framework presented in Ning et al. (2016). We have incorporated this vital reference into our introduction and uncertainty analysis sections to provide a much more balanced and scientifically accurate picture of the end-to-end GNSS-IWV error budget. Furthermore, we strongly agree with the reviewer's practical insight regarding the high capital investment required for radiometric hardware; we have added explicit commentary in our conclusion emphasizing that this approach is best targeted at specific climatological hotspots rather than universal deployment. Throughout the revised manuscript, we have eagerly adopted the reviewer's suggested wordings to thoroughly nuance our claims.

Rain is only mentioned in terms of extreme weather in the introduction, but it is not pointed out that the microwave radiometer algorithms more or less break down during rain. I assume that the station on Cyprus may not be exposed to rain during a large percentage of time, and perhaps the extreme temperature profiles (difficult to model based on ground temperature only) which the study is focused on, do never occur during rain? In any case, I think it is important to mention the poor accuracy of the radiometer retrievals during rain.

Response: We thank the reviewer for highlighting this critical operational limitation of microwave radiometry. The reviewer is absolutely correct that MWR retrieval algorithms degrade significantly during precipitation events due to liquid water emission and scattering on the radome. As the reviewer correctly hypothesized, the hydro-climate of Nicosia during our study period (the warm season, March to October 2025) is characterized by a prolonged dry season, meaning rainfall events were exceedingly rare and did not coincide with the extreme thermodynamic profiles under investigation. To ensure these wet-radome errors did not contaminate our dataset, we utilized the co-located Vaisala WXT536 weather transmitter (which records rain accumulation and duration) to identify and filter out any highly infrequent rainy epochs prior to our statistical analysis. We agree that this quality-control step and the inherent limitations of MWRs during rain should be explicitly stated for transparency. We have updated Section 2.2.1 to reflect this.

Specific comments

Line (L) 10: fundamentally limited --> significantly affected
Response: Corrected in the revised manuscript.

L 23: essential --> meaningful
Response: Corrected in the revised manuscript.

L 57: Here you state that "Tm is the primary source of uncertainty in GNSS meteorology after ZTD estimation". It will be appropriate to either (1) also here point out that the uncertainty in the ZTD is larger or comparable when it comes to the impact on the uncertainty of the final IWV, or (2) replace "the primary" with "one".
Response: The reviewer is entirely correct that the uncertainty originating from the ZTD estimation itself—particularly from mapping functions and the interpolation of surface pressure required for Zenith Hydrostatic Delay (ZHD)—frequently matches or exceeds the error introduced by the Tm conversion factor. While our specific CYGMEN site mitigates much of the ZHD error via a co-located pressure sensor (as detailed in Section 3.4.3), we agree that the generalized statement in the introduction needed to be refined for broader scientific accuracy. We have adopted the reviewer's suggestion by replacing "the primary" with "a primary" and adding a clause explicitly acknowledging that ZTD uncertainties are often comparable or larger..

L 60: Assuming that 1-2 % error corresponds to 1-2 mm in the PWV implies that the value of the PWV is 100 mm. I am not sure if such a high value has ever been observed. Why not just state the percentage uncertainty and let the reader figure out what it means in absolute value given the weather conditions.
Response: We sincerely thank the reviewer for catching this mathematical oversight. The reviewer is absolutely correct: equating a 1-2% relative error directly to a 1-2 mm absolute error implies an

unrealistic baseline PWV of 100 mm, which does not occur in nature. We have removed the erroneous absolute equivalent from the manuscript and adopted the reviewer's suggestion to state only the percentage uncertainty, allowing it to scale properly with the ambient meteorological conditions.

L 84: It will be interesting to know how many sites with co-located MWR and GNSS there are in the network?

Response: We thank the reviewer for pointing this out; we realize our original phrasing may have been ambiguous regarding the network's scale. While the CyMETEO network consists of a dense array of continuous GNSS stations distributed across the island, there is currently only one site equipped with a co-located Microwave Radiometer (MWR) and Radiosonde (RS) facility. Because MWRs are high-cost and operationally complex instruments, this single co-located setup at the Athalassa observatory serves as the project's central thermodynamic "supersite." We have revised the manuscript to explicitly clarify the number of MWR instruments and the exact architecture of the network.

Table 1: It will make sense to also state that the MWR "Role in Study" is also to provide the IWV.

Response: We appreciate the reviewer pointing this out. Given that Section 3.2 extensively utilizes the MWR-derived IWV as a high-quality comparative standard against both the radiosonde and GNSS retrievals, it is entirely appropriate to list IWV estimation as one of the instrument's primary roles. We have updated Table 1 accordingly to provide a more complete summary of the MWR's utility in this study.

Table 1: You state that the humidity uncertainty of the RS humidity sensor is 4 %. This deserves some comment in the text when you use this sensor as "ground truth" given that you evaluate uncertainties at comparable and lower levels.

Response: The 4% uncertainty listed in Table 1 represents the manufacturer's specified uncertainty for individual, point-wise RH measurements. However, because our validation primarily relies on column-integrated quantities (IWV) and weighted vertical integrals (Tm), the random, uncorrelated measurement noise at discrete vertical levels is significantly suppressed during the mathematical integration process. While systematic sensor biases could theoretically persist, the Vaisala RS41 is widely established in the literature as the operational gold standard for in-situ profiling, with excellent systematic stability. We have added a brief discussion in Section 2.2.2 to explicitly address how error propagation during integration allows us to confidently utilize the RS as a "ground truth" benchmark for evaluating smaller relative uncertainties in the MWR and GNSS retrievals.

L 155: The IGS Ultra-Rapid products are not the optimal choice when aiming at the lowest possible uncertainty which should be the case for climate studies. Please comment on the impact of this choice and why it was done this way.

Response: The reviewer is entirely correct that for post-processed, long-term climate trend analysis, IGS Final products are the optimal and necessary choice. Our decision to utilize IGS Ultra-Rapid products was driven by our secondary objective to evaluate the proposed synergistic MWR-GNSS architecture under simulated real-time operational conditions. As discussed in our introduction, a major application of high-resolution PWV is the assimilation into NWP models for short-range precipitation "nowcasting." By using Ultra-Rapid products, we aimed to demonstrate that replacing static Tm models with real-time MWR data provides massive accuracy gains even when constrained by operational, near real-time orbital products. Regarding the impact: the degradation in ZTD accuracy when switching from Final to Ultra-Rapid products is typically on the order of a few millimeters, translating to less than 0.5 mm in PWV. As our uncertainty analysis demonstrates, this orbital/clock uncertainty is an order of magnitude smaller than the severe systematic biases (> 1.0 mm PWV) induced by the static empirical thermodynamic models during peak heating. Nevertheless, we agree this rationale and caveat must be explicitly stated. We have updated Section 2.2.3 to clarify this choice and its minimal impact on our thermodynamic error diagnosis.

L 182: What are the estimated values of alpha and beta?

Response: We thank the reviewer for pointing out this omission. The specific numerical values of the regression coefficients α (1.0623) and β (-15.6062) were inadvertently left out of the initial manuscript draft. We have now updated Section 2.3 to explicitly state the derived parameters used for the linear bias correction model.

Figure 4(b): Which estimate of Tm is used in this comparison?

Response: We thank the reviewer for pointing out this missing detail. For the GNSS IWV calculations shown in Figure 4(b) (and 4(c)), the weighted mean temperature (Tm) was derived using the static HGPT2 empirical model, representing our "Standard Retrieval" baseline. We apologize for not making this explicit in the figure's caption. We have updated the caption for Figure 4 to clearly specify the source of the Tm used for the GNSS data.

L 299: This result is already stated in L 288.

Response: The reviewer is completely correct; the concluding sentence was an artifact of the drafting process and merely repeated the finding that had already been established earlier in the section. We have deleted the redundant sentence to improve the flow and conciseness of the paragraph.

L 341: Here again it is implicitly assumed that the error in the ZTD is zero, which is not true. Please rewrite.

Response: We appreciate the reviewer's in catching this phrasing. Similar to the earlier comment regarding the introduction, our text inadvertently implied a flawless ZTD estimation by focusing solely on the thermodynamic conversion step. We completely agree that the total PWV error is a combination of both ZTD estimation errors and Tm inaccuracies. We have rewritten this section to explicitly state that the linear dependency on Tm applies specifically to the conversion factor (Π), under the assumption of a given ZTD, rather than representing the absolute entirety of the PWV error budget.

Figure 9: This figure does not give the whole picture. Even if you do not include additional errors in the figure, I think it is necessary to mention them. I am thinking of orbit and clock errors, as well as mapping function, and site dependent (the electromagnetic environment such as reflections/multipath) errors in the GNSS processing.

Response: The reviewer is absolutely correct that Figure 9 isolates only the uncertainties associated strictly with the conversion factor (Π) and does not represent the full, end-to-end error budget of the GNSS PWV retrieval. While the specific focus of this component-wise analysis was to contrast thermodynamic assumptions against physics constants, we completely agree that it is necessary to explicitly acknowledge the geodetic and site-dependent errors that affect the Zenith Total Delay (ZTD) estimation prior to this conversion step. We have revised Section 3.4.3 to explicitly mention orbit and clock errors, mapping function uncertainties, and site-dependent electromagnetic effects (such as multipath), providing the reader with a fully transparent overview of the total error budget.

L 475: Give a reference for the "known artifact".

Response: Provided the references revised manuscript

L 510 - 514: "This correction effectively halves the uncertainty in the final GNSS water vapor product compared to standard climatological approaches." is not true, and it is not "the dominant error source in GNSS meteorology". These statements ignore the errors in the ZTD from the GNSS processing. When rewriting this text I suggest that you instead use words such as "may be comparable to", "are significant", or something similar. In this context I think you also have to point out that there are many sites world-wide where the use of microwave radiometry may not be motivated (given the high cost, all the other uncertainties, and that models for Tm is not that poor everywhere).

Response: We sincerely thank the reviewer for this crucial final check on our concluding statements. The reviewer is absolutely right that our original concluding phrasing inadvertently dismissed the substantial errors inherent in the ZTD estimation itself (orbits, clocks, mapping functions), thus overstating the relative impact of the Tm conversion step on the *total* error budget. We have revised the conclusion to adopt the reviewer's more precise terminology, stating that Tm errors are "significant" and "comparable to" ZTD uncertainties. Furthermore, we strongly agree with the reviewer's point regarding the global applicability of this method. We have added a dedicated caveat to the conclusion explicitly stating that given the high operational cost of MWRs, and the fact that empirical Tm models perform perfectly adequately in many regions without complex coastal thermodynamics, universal deployment of this synergistic architecture is not motivated. We thank the reviewer for helping us ground our conclusions with this important operational context.

Technical Corrections

L 27 + more: You use the American spelling of vapour, although ACP is a European journal?

Response: We have performed a comprehensive find-and-replace throughout the entire manuscript, including the title, to ensure the British spelling ("vapour") is used consistently.

L 30: Here you introduce PWV (unit mm). Later (L 118) you also use IWV (unit kg/m^2). You should use only one of these in order not to confuse the reader. I suggest to chose IWV because the delays from GNSS is expressed in unit of length and it makes it easier to see the difference.

Response: Our rationale is that in the meteorological, climatological, and severe-weather forecasting communities—which are the primary end-users of this GNSS data—PWV in millimeters is the universally recognized standard, as it intuitively corresponds to precipitation depth. Furthermore, the foundational literature in GNSS meteorology (e.g., Bevis et al., 1992) established the convention of mapping Zenith Wet Delay (in mm) directly to PWV (in mm) via the dimensionless conversion factor (Π). However, we fully agree with the reviewer that introducing both IWV (kg m−2) from the radiometer/radiosonde integrations and PWV (mm) from the GNSS could confuse readers unfamiliar with the terms. To resolve this, we have added an explicit clarifying statement in the methodology section confirming that the two terms describe the exact same physical quantity and are numerically equivalent (1 kg m−2 = 1 mm), allowing us to maintain meteorological conventions while completely eliminating any ambiguity.

L 34: Here the acronym EM is defined, but it is defined again on L 464. At many other places you do not use the acronym. The acronym is only used 9 times, so I suggest not to use it at all. I think it will make the reading better.

Response: We completely agree that low-frequency acronyms can disrupt the flow of the text, and the inconsistent use and double-definition of "EM" was an oversight on our part. We have adopted the reviewer's suggestion and removed the "EM" acronym entirely. We have performed a global replace throughout the manuscript to spell out "Eastern Mediterranean" in all instances.

L 60 + more: 2% --> 2 % (SI recommendation)
Response: Corrected in the revised manuscript.

Table 1: 1 sec --> 1 s (SI recommendation)
Response: Corrected in the revised manuscript.

L 114: Training Set --> training set
Response: Corrected in the revised manuscript.

L 115: Validation Set --> validation set
Response: Corrected in the revised manuscript.

L 132: delete "total column" (it is already in the definition of the IWV)
Response: Corrected in the revised manuscript.

L242: 6.7°C --> 6.7 °C
Response: Corrected in the revised manuscript.

L 284: whereas the MWR exhibits --> whereas it relative to the MWR exhibits
Response: Corrected in the revised manuscript.

L 560, 567: doi missing
Response: Corrected in the revised manuscript.

L 589: the doi link is not correct, shall be:
Response: Corrected in the revised manuscript.

https://link.springer.com/book/10.1007/978-3-030-13901-8, and the author list is not correct. (Note that I have not checked all the doi links, so it may be a good idea for you to carry out?)
Response: Corrected throughout in the revised manuscript.

L 621: add https://doi.org/10.1175/1520-0442(1996)009%3C3561:TWVCAT%3E2.0.CO;2
Response: Corrected in the revised manuscript.