

You haven't taken into account the suggestions made by the third reviewer in her/his report. Haven't you noticed this review report? Please find it here below:

**Reply:** We thank the Editor/Reviewer for bringing this to our attention. We sincerely apologize for the omission. The report from the third reviewer did not appear to be included in the previous decision letter we received. However, if it was indeed attached and we inadvertently overlooked it, we are truly sorry for the oversight. We greatly appreciate the third reviewer's valuable insights and will thoroughly address all of their suggestions in the revised manuscript.

### **\*\*General Comments\*\***

This article by Parde et al. proposes a method for computing the mean temperature of the wet water vapor column,  $T_m$ , for the conversion of GNSS ZWD into PWV / IWV. The method is based on the use of humidity and temperature profiles from a colocated WVR. The proposed solution is original and quite comprehensive, with in particular a rather thorough investigation of errors. However, I am not able to see its operational interest: if one has an operational radiometer (i.e. providing usable measurements: no rain for example), why would one want to compute GNSS IWVs? Or is the idea to apply this  $T_m$  over an entire region (for example the whole island?) Could you clarify this point in the conclusion, for example? This is a fairly complete article, well written and properly argued (despite a few points mentioned below): I recommend acceptance of this article after minor revision taking into account my comments.

**Reply:** We sincerely thank the Reviewer for their positive overall assessment of the manuscript and for highlighting this crucial operational point. We agree that the practical, operational utility of this synergistic approach needs to be explicitly articulated.

The operational interest lies in a combination of cost, spatial density, and all-weather capability:

1. **Cost and Density:** As noted in the manuscript, MWRs involve high capital and maintenance costs, making it unfeasible to deploy them in dense arrays. GNSS receivers, conversely, are highly cost-efficient and can be deployed densely across complex terrain to capture mesoscale variations.
2. **All-Weather Capability:** Passive MWR retrievals degrade significantly during precipitation events due to wet-radome contamination. GNSS signals are unaffected by rain, allowing for continuous severe weather monitoring.
3. **Regional Extrapolation:** The operational vision—as the Reviewer correctly hypothesized—is to use a single, centralized MWR "supersite" to capture the complex, real-time diurnal thermodynamic variations that static models miss. These highly accurate, MWR-derived diurnal  $T_m$  parameters (or shape functions) can then be extrapolated to correct static models across a broader regional network of cheaper GNSS receivers where direct MWR collocation is impossible.

We appreciate the suggestion to make this clearer. We have added a dedicated paragraph to the Conclusion to explicitly clarify this operational strategy.

## **\*\*Specific comments\*\***

Lines 54–57: redundant: if the parameters are linearly proportional, it is obvious that an n% relative error on one implies an n% relative error on the other. These sentences could be simplified, I think.

**Reply:** We agree with the Reviewer that the detailed breakdown of the relative error propagation was redundant. Since the linear proportionality is already established, explicitly stating the 1% to 1% error translation is unnecessary. We have simplified these sentences in the revised manuscript to be more concise.

Line 152 (Eq. 3): is there not a risk of biased IWV computation with trapezoidal integration? (an exponential curve, for example, would systematically lie below a trapezoid, therefore leading to a risk of overestimation). This risk could be reduced if the vertical resolution of RS is fine. Is it?

**Reply:** The Reviewer makes a very valid theoretical point regarding the truncation error of the trapezoidal rule over exponential profiles. Fortunately, the vertical resolution of our in-situ profiles is extremely fine, which effectively eliminates this risk. The Vaisala RS41-SGP instruments provide 1-second telemetry, resulting in a vertical layer spacing of approximately 5 to 8 meters during ascent. With such small integration steps, the linear approximation between consecutive levels is virtually identical to the true exponential curve, rendering any systematic overestimation negligible. A note regarding the vertical resolution has been added to the methodology section to clarify this for readers.

Lines 165–173: did you perform a quality check of the radiosondes (do they all reach 10 km?) Is the horizontal drift not too large (it could cause discrepancies in the upper layers)?

**Reply:** We appreciate the Reviewer's insightful questions. Yes, a strict quality control filter was applied to the radiosonde data. Because the standard burst altitude of our launches is ~30 km, the vast majority of profiles easily exceeded our 10 km integration threshold. Any anomalous flights that failed to reach 10 km were excluded to ensure a one-to-one vertical comparison with the MWR.

Regarding horizontal drift, we agree that it causes spatial discrepancies in the upper layers. The radiosonde naturally drifts away from the MWR's strict zenith column as it ascends. This spatiotemporal mismatch is precisely why we observe an increase in random scatter (RMSE) in the free troposphere, as we briefly noted in the manuscript. Fortunately, this drift introduces random rather than systematic error, meaning our mean bias corrections and overall conclusions remain statistically robust.

Line 179: it is also possible to assimilate GNSS ZTD directly. This also avoids having to deal with the estimation of  $T_m$ ...

**Reply:** We thank the Reviewer for this insightful observation. It is absolutely correct that many modern operational Numerical Weather Prediction (NWP) systems prefer to assimilate Zenith Total Delay (ZTD) directly, which elegantly bypasses the uncertainties associated with  $T_m$  estimation and conversion. However, providing a highly accurate, physical Precipitable Water Vapour (PWV) product remains critical for applications outside of direct model assimilation. Specifically, operational forecasters rely on PWV as an intuitive, physical metric for real-time situational awareness and severe weather "nowcasting," whereas a raw delay value is not easily interpreted on a synoptic weather map. Furthermore, for climatological applications, generating

long-term historical records requires physical moisture variables to accurately track the regional hydrological cycle and allow for direct intercomparison with other atmospheric profiling instruments. We have added a brief clarification to the manuscript to acknowledge direct ZTD assimilation while justifying the necessity of the PWV conversion.

Lines 234–241: I do not understand why you switch from PWV to IWV here. Could everything be done with PWV? Why use both quantities in the text?

**Reply:** We thank the Reviewer for highlighting this inconsistency. We agree that switching between IWV and PWV creates unnecessary confusion for the reader. While the terms were initially separated to distinguish between specific measurement conventions (mass integration vs. water depth), we have now standardized the terminology. All instances of "IWV" have been replaced with "PWV" throughout the revised manuscript for consistency exclude section 2.3 and 3.2.

Section 3.4.2: did you evaluate the temporal stability of this calibration?

**Reply:** We appreciate the Reviewer's question. The temporal stability was indeed evaluated and confirmed via our chronological dataset split. As mentioned in Section 2.2.1, the regression model was trained on data from April–June and independently validated on unseen data from July–October. The fact that the correction successfully reduced the systematic bias from -1.90 K to 0.50 K on this independent, late-summer validation set proves that the calibration remains stable over time and is not overfitted to a specific month. We have added a clarifying sentence to Section 3.4.2 to explicitly highlight this temporal stability.

Line 461: I was not able to find the article you cite (Van Malderen et al. 2022): the DOI points to another article and Google does not return any paper with the exact title you cite...

**Reply:** We thank the Reviewer for their diligent check of our references. We have double-checked the citation, and the article by Van Malderen et al. (2022) is correct as cited. It was published in the journal *Remote Sensing* (Volume 14, Issue 4, Article 1050), and the DOI successfully resolves to the correct page. We suspect there may have been a temporary routing error with the DOI service when the link was accessed.

For clarity, the exact details of the paper are as follows:

- Title: Global Spatiotemporal Variability of Integrated Water Vapor Derived from GPS, GOME/SCIAMACHY and ERA-Interim: Annual Cycle, Frequency Distribution and Linear Trends.
- DOI: <https://doi.org/10.3390/rs14041050>

Conclusion: what is the applicability of the method in an operational context?

**Reply:** We thank the Reviewer for highlighting the need to explicitly state the operational applicability of our work. To address this, we have expanded the Conclusion section to outline exactly how this synergistic method functions in a real-world forecasting environment. Specifically, we emphasize the "supersite" or "anchor station" concept: utilizing a single, centrally located microwave radiometer to dynamically correct T<sub>m</sub> models for an entire dense, low-cost

regional GNSS network. This allows national meteorological services to generate the high-fidelity PWV fields required for real-time severe weather nowcasting without the prohibitive cost of deploying multiple radiometers.