

## Response Letter to Reviewer #2

**Dear Reviewer,**

We would like to express our sincere appreciation for your thoughtful and constructive feedback on our manuscript titled “*Evaluation of reanalysis precipitable water vapor under typhoon conditions using multi-source observations*” (Manuscript ID: EGUSPHERE-2025-4438). We are grateful for the time and effort you dedicated to reviewing our work.

We have carefully considered all of your comments and suggestions and revised the manuscript accordingly. All modifications have been clearly marked using “Track Changes.” As we made substantial and detailed revisions throughout the manuscript, the tracked-changes version may be less convenient to read. Therefore, we have also provided a clean version (with all changes accepted) for your reference. In this response letter, we first provide a general response of the major revisions. Then, we respond to each comment point by point and indicate the corresponding line numbers in the clean version to facilitate your review. In addition to addressing the specific comments, we also checked the manuscript for overall consistency in terminology, notation, and figure/table references.

### ***General response:***

A major revision in the revised manuscript concerns the typhoon track data source. In response to Major Comment #2, where you and another reviewer raised the same concern, you suggested using tropical cyclone (TC) information from either the International Best Track Archive for Climate Stewardship (IBTrACS) or the China Meteorological Administration (CMA). We therefore downloaded and carefully examined both datasets. For CMA, we first checked the Best Track Data under the National-level Operational Data Product provided by the CMA Tropical Cyclone Data Center. The CMA best-track dataset covers our study period from January 2020 to December 2024 and includes most of the typhoon-related information needed for this work. Meanwhile, in Major Comment #4, you noted that the criterion for determining whether a station is within a typhoon’s influence should be refined, and that a single fixed distance is insufficient. Implementing a more physically sound, typhoon-size-dependent collocation scheme based on wind radii requires wind-radii information. After careful checking, we found that wind-radii information is not included in the CMA best-track product. We then examined the TC Size Analysis product under the Scientific Research Data Products released by the CMA Tropical Cyclone Data Center, including both the Tropical Cyclone Size Dataset (v3.0) and (v2.0), and confirmed that these datasets contain wind-radii information. However, these size datasets are currently available only up to 2020 and therefore do not cover

our full analysis period (2020–2024). As a result, we were unable to use the CMA products to implement a consistent wind-radii-based dynamic collocation scheme for the entire study period. We then checked IBTrACS and confirmed that it provides the wind-radii information required for dynamic collocation. We therefore adopted IBTrACS as the typhoon dataset used in this study. Details of the IBTrACS data and the dynamic collocation scheme are provided in our point-by-point responses to Major Comments #2 and #4.

Based on the IBTrACS data, we re-ran all experiments in this study, comprehensively re-examined the updated results. Some values may differ slightly from those reported in the original manuscript, but our main results have not changed.

We substantially revised the results section with more detailed explanations where appropriate. We also carefully reviewed the reference list and in-text citations and corrected cases where references were inaccurate, inappropriate, or redundant. In addition, we ensured consistency in terminology, notation, and writing style, corrected errors, and improved the overall language. To strengthen the linkage between the figures/tables and the main text, we substantially revised the narrative structure so that each conclusion is more explicitly supported by the corresponding evidence, including clearer references to figures, tables, and specific panels. We also replotted nearly all figures and standardized the figure numbering and panel-labeling style to improve readability and consistency. Finally, we added clarifications and additional explanations where the original text was ambiguous or insufficiently justified. Point-by-point responses are provided below.

### ***Point-by-point responses for major comments:***

#### **Major Comment #1**

The introduction currently contains many short paragraphs, which fragments the narrative and makes it difficult to follow the overall logic. I recommend merging these paragraphs to create a more cohesive and flowing argument.

#### **Response #1 (Line 29–102)**

Thank you for your important overall comment on the Introduction. We carefully reconsidered the logical structure of the Introduction and merged several paragraphs to improve coherence and readability. The revised structure is as follows. The first paragraph highlights the importance of atmospheric water vapor and precipitable water vapor (PWV) research. The second paragraph focuses on the TC, briefly introduces TCs and typhoons, and emphasizes the importance of water-vapor-related studies in TC/typhoon research. The third paragraph summarizes common approaches for estimating or retrieving PWV and introduces the three reanalysis datasets evaluated in this study. The fourth paragraph reviews the current status and recent

progress in PWV estimates derived from reanalysis products. The final paragraph briefly summarizes our work and outlines the structure of the paper.

### **Major Comment #2**

It is recommended to use a TC best-track dataset, such as the IBTrACS (covering the whole globe) or the CMA TC best-track dataset (covering the Northwest Pacific), as the observational data for TC tracks and intensity.

### **Response #2 (Line 107–119)**

Thank you for your comment on the typhoon track data. We fully agree that using an authoritative TC best-track dataset is more appropriate and reliable for this type of study. Another reviewer also suggested using typhoon data from the CMA Tropical Cyclone Data Center. As noted in the general response, we have addressed this issue overall. Here we respond in more detail. Following your suggestion, we downloaded and carefully examined the best-track datasets from both IBTrACS and the CMA Tropical Cyclone Data Center. Since you recommended in Major Comment #3 that we adopt a dynamic collocation approach based on typhoon wind radius, wind-radius information is required for our revised collocation strategy. After careful checking, we found that the CMA Tropical Cyclone Data Center best-track product does not provide wind-radius variables. We therefore used the global TC dataset from IBTrACS. Details of the wind-radius-based dynamic collocation between the typhoon track and GNSS stations, radiosonde stations, and RO profiles are provided in our response to Major Comment #3.

Because we changed the typhoon dataset, we repeated all experiments in this study, recomputed the statistics, and replotted most figures and tables. It is therefore expected that some values differ from those reported in the original manuscript, and we have comprehensively re-examined and re-analyzed the updated results. It is important to note that our main results have not changed.

### **Major Comment #3**

The interpolation method used to collocate the reanalysis data with the GNSS, radiosonde, and RO observation points must be clearly specified.

### **Response #3 (Line 242–247)**

Thank you for your comment on the interpolation method. In the revised manuscript, we have added a clear description in Section 2.6, “Data collocation schemes and PWV horizontal interpolation method”, explaining how REA-PWV is obtained from the reanalysis datasets at GNSS stations, radiosonde stations, and the mean tangent point (observation point) of each RO profile. Specifically, we now describe the bilinear horizontal interpolation approach and provide the corresponding

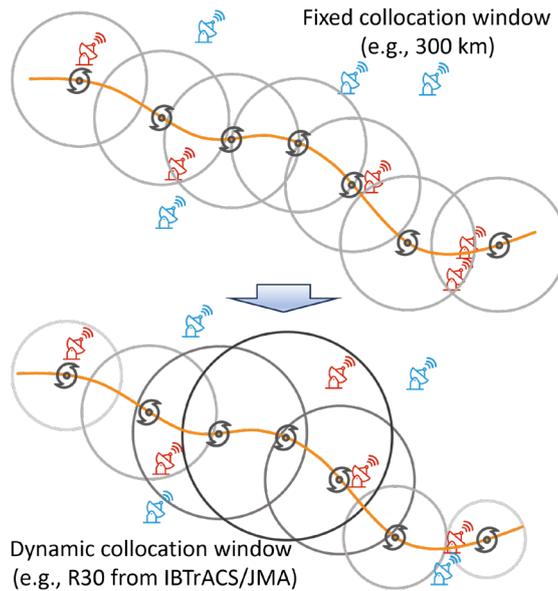
formula. We also removed the previous description of vertical adjustment, because PWV vertical adjustment is no longer applied in this study. This change is further explained in our response to Minor Comment #14.

#### **Major Comment #4**

Section 2.5: The criteria for determining if a station is within a TC's influence should be refined. Using a single, fixed distance from the TC center is insufficient. A more dynamic threshold, based on the TC's intensity and structure (e.g., the radius of force 7 winds) at the specific time, would be more physically sound. This same consideration applies to the collocation of RO profiles.

#### **Response #4 (Line 231–241)**

Thank you for your constructive and important comment on the collocation scheme. We fully agree that using a single fixed spatial collocation window is insufficient, and that a more dynamic threshold is physically more appropriate. In response, we introduced a new dynamic collocation approach based on the typhoon wind radius in Section 2.6 of the revised manuscript, “Data collocation schemes and PWV horizontal interpolation method”. Specifically, for the western North Pacific, the TC best track information in IBTrACS is primarily provided by the Japan Meteorological Agency (JMA), as indicated by the WMO\_AGENCY field being “tokyo.” For wind-radii variables, JMA provides TOKYO\_R30 and TOKYO\_R50, and TOKYO\_R30 is closer to the radius of gale-force (force 7) winds. We therefore used TOKYO\_R30 to define the dynamic spatial collocation window. TOKYO\_R30 includes TOKYO\_R30\_LONG and TOKYO\_R30\_SHORT, which represent the longest and shortest radii of winds of 30 kt or greater. We took the average of TOKYO\_R30\_LONG and TOKYO\_R30\_SHORT as the dynamic collocation radius threshold and used it to collocate the typhoon-track points with GNSS stations, radiosonde stations, and RO profiles. For temporal collocation, the procedures for GNSS and radiosonde data are consistent. For each typhoon-track time, we identified the two nearest observations before and after that time and linearly interpolated PWV to the typhoon-track time. For RO profiles, we used a 30-minute temporal window. An RO profile is considered collocated when its observation time differs from the corresponding typhoon-track time by no more than 30 minutes. A schematic illustrating the change from the fixed spatial-window collocation in the original version to the current dynamic collocation is shown in the figure below. Red stations indicate ground-based stations collocated with the typhoon track, whereas blue stations indicate stations not collocated with the typhoon track.



Because the collocation scheme was refined and the typhoon dataset was updated, some values in the revised manuscript differ from those in the original version. This is expected after the methodological updates, and we have thoroughly rechecked and discussed the updated results. Our main results have not changed.

#### Major Comment #5

Section 3.1.2: From a technical standpoint, if the aim is to account for TC genesis and residual circulation, a period of 2-3 days for the AP should be sufficient.

#### Response #5 (Line 325)

Thank you for your suggestion regarding the length of the Adjacent Period (AP). Following your recommendation, we revised the AP to three days before and three days after the typhoon influence period, and we updated the relevant statistics, figures, and text in Section 3.1.2 accordingly. This adjustment makes the comparison between typhoon and non-typhoon periods more focused. It also makes Figure 4 (Figure 3 in the original manuscript) clearer and more concise, allowing a more straightforward discussion of the results.

#### Major Comment #6

Section 3.2: The comparison between TC and non-TC conditions is missing for the radiosonde data.

#### Response #6 (Line 420–430)

Thank you for pointing out that the comparison between typhoon and non-typhoon conditions using radiosonde data was missing in Section 3.2. We have added the non-typhoon evaluation results with radiosonde as the reference in the revised manuscript and compared and discussed them alongside the results during

typhoon periods, which makes this section more complete.

#### **Major Comment #7**

Section 3.3: Errors of REAs are larger under TC conditions than non-TC conditions using RO as reference, which is the opposite of what is observed with GNSS as reference. The authors should explain this contrasting behavior.

#### **Response #7 (Line 459–463)**

Thank you for pointing out this seemingly contrasting result and the need for an explanation. We suggest that this difference is likely related to the spatial sampling characteristics and sample size of the different observations. Specifically, the RO profiles used for evaluation in this study are predominantly over the ocean, and their land–sea distribution differs substantially from that of the ground-based GNSS and radiosonde stations. As a result, the comparison of errors between typhoon and non-typhoon periods may exhibit different statistical behavior. In addition, the number of RO profiles available for evaluation is relatively limited, so the statistics are more sensitive to individual events and small samples, which can lead to differences compared with the conclusions based on GNSS. Moreover, although we selected “high-quality” RO profiles (with the lowest altitude below 500 m), the uncertainty characteristics of RO moisture retrievals can still differ from those of GNSS and radiosonde observations in some situations. This may also affect the typhoon versus non-typhoon error comparison when RO is used as the reference. Given that the main conclusions of this study are based on the ground-based GNSS, which provides a larger sample size and higher temporal resolution, the RO-based results are mainly used as an independent supplementary check in this paper. We have added the above explanation in Section 3.3 of the revised manuscript and clarified that this contrasting behavior may be related to sampling and sample-size effects. We will investigate the causes of this difference in future work.

#### **Major Comment #8**

Section 3.4: The equation used to calculate the NSD should be explicitly provided in the manuscript. Additionally, TCs are asymmetric systems, therefore, large inconsistencies between neighboring grids (a high NSD) do not necessarily indicate a large error in the reanalysis product.

#### **Response #8 (Line 269)**

Thank you for your key comments on the NSD. We have added the definition and formula for calculating NSD in Section 2.7, “Statistical metrics”, and we also explain its physical meaning and the calculation procedure in the text. We also fully agree with your second point that TCs are highly asymmetric systems with strong

gradients, so large inconsistencies between neighboring grids, indicated by a high NSD, do not necessarily imply a large error in the reanalysis product. Based on this consideration, we revised the relevant text in Section 3.4. We no longer interpret NSD directly as evidence of error magnitude. Instead, we use it more cautiously as an indicator to characterize the amplitude of spatial variations and local inhomogeneity of the reanalysis fields under TC conditions, and to compare the spatial structure features among different reanalysis products during typhoons. We also re-examined and updated the results and discussion in this section, and we emphasize that NSD should be interpreted together with other error metrics and observational references to avoid overinterpretation.

### **Major Comment #9**

The discussion section contains new results that should belong in the results section. The discussion should be revised to interpret existing findings, not introduce new ones. Additionally, this analysis does not address the major methodological concern raised earlier regarding the use of a single, fixed distance threshold for selecting stations within a TC's range.

### **Response #9**

Thank you for your key suggestions regarding the structure and content of the discussion. We agree that the discussion should focus on interpreting and expanding upon the existing results rather than introducing new findings. Following your recommendation, we merged the discussion and conclusion in the revised manuscript, and we rewrote and streamlined this section. On the one hand, we removed the result-like content that was newly presented in the original discussion (e.g. fig. 8 and the discussion around it) and moved it to the appropriate results subsections, and we retained only the interpretation, comparison, and implications of the findings already shown in the results. On the other hand, regarding the earlier methodological concern about using a fixed distance threshold to define a station within a typhoon's influence, we have adopted a more physically meaningful dynamic collocation threshold in the revised methods and updated all analyses accordingly. The related discussion has therefore been revised to align with the updated collocation scheme and the corresponding results, which makes the overall structure and reasoning of the manuscript more consistent.

### ***Point-by-point responses for minor comments:***

#### **Minor Comment #1**

Lines 125-126 “Specifically ...”: The language used here is imprecise and should be revised for scientific rigor.

### **Response #1 (Line 124–128)**

Thank you for pointing out this imprecise wording. We have revised the text in the manuscript and removed the phrasing that was not sufficiently rigorous.

### **Minor Comment #2**

For clarity, please list only the three reanalysis products (i.e., ERA5, MERRA-2, JRA-55) in the first row.

### **Response #2 (Line 157)**

Thank you for pointing out this issue. Although you did not specify the exact location, we understood it as referring to the “Data description” row in Table 1, and we revised it accordingly to include only ERA5, MERRA-2, and JRA-3Q as requested. If this is not the intended location, we would be grateful if you could indicate where further revision is needed, and we will revise it accordingly.

### **Minor Comment #3**

Details regarding the visual content of figures (e.g., Lines 269-271 and Lines 303-309) should be moved from the main text into the corresponding figure captions.

### **Response #3 (Line 285–291 & 325–329)**

Thank you for noting that the detailed descriptions of Figure 2 in the original manuscript (Figure 3 in the revised manuscript) and Figure 3 in the original manuscript (Figure 4 in the revised manuscript) were redundant in the main text. We have moved these detailed descriptions to the corresponding figure captions. We also reviewed other figures for the same issue and made similar revisions where necessary.

### **Minor Comment #4**

Lines 278: The distinction between REA-PWV<sub>c</sub> and REA-PWV<sub>i</sub> should be explained. Is this primarily due to the spatial distribution of GNSS stations from these two networks?

### **Response #4 (Line 295–296)**

Thank you for your concern. Yes, we agree with your view that the differences between REA-PWV<sub>c</sub> and REA-PWV<sub>i</sub> are mainly driven by the different station distributions. In this study, we used 32 IGS stations and 29 CMONOC stations. The station counts differ from those in the original manuscript because we adopted a new collocation scheme. Because the spatial distributions of these two GNSS networks are different, the corresponding results also differ.

### **Minor Comment #5**

Line 284: Remove “from the GNSS ground-based observations”.

### **Response #5**

Thank you for your suggestion. We removed “from the GNSS ground-based observations.”

### **Minor Comment #6**

Lines 287-288: RB is essentially bias over PWV, and its pattern is dominated by PWV distribution.

### **Response #6**

Thank you for your comment. We fully agree. In the revised manuscript, we rewrote the analysis related to RB and removed this redundant statement.

### **Minor Comment #7**

Line 290: The number of stations is used in the computation of both RMSE and bias. This statement needs to be revised.

### **Response #7 (Line 307–309)**

Thank you for your suggestion. We revised this part to avoid ambiguous wording in the original manuscript. It now reads: “network-weighted mean bias and RMSE are obtained by combining the CMONOC-based and IGS-based results, with weights determined by each network’s proportion of the total number of used GNSS stations”. This clarification indicates that the weights are determined by the station counts of the two GNSS networks, and that the weights themselves do not enter into the calculation of the bias, RMSE, or other statistical metrics.

### **Minor Comment #8**

Line 293: Add “among the REAs” after “are the largest”.

### **Response #8 (Line 313)**

Thank you for your suggestion. We added “among the three reanalyses” after “are the largest”.

### **Minor Comment #9**

Figure 2: The figure currently contains too much information, making it difficult to interpret. To improve clarity, I recommend that the results for the individual TC categories be moved to the supplementary material.

### **Response #9 (Line 324)**

Thank you for your suggestion regarding the readability of Figure 2. Because the results for different TC categories mentioned in your comment are not presented in Figure 2 of the original manuscript, we understand that you may be referring to Figure 3 in the original version. We agree that this figure contained too much information

and was difficult to interpret. Following major comment #5 on the AP setting, we shortened the AP from 7 days before and after the TC period to 3 days before and after, and we re-calculated and updated the corresponding results and figures accordingly. This adjustment substantially reduced the amount of information that needs to be shown in the figure and clarified the comparisons among TC categories, and the relevant panels in Figure 3 (Figure 4 in the revised manuscript) have been simplified accordingly. We have updated the figure captions and the related text in the manuscript to reflect these changes. If you still consider it preferable to move the results for individual TC categories to the Supplement, we would be happy to revise the presentation accordingly.

#### **Minor Comment #10**

Section 3.1.3: The equation used for the computation of dRMSE should be provided.

#### **Response #10 (Line 253)**

Thank you for your suggestion. In the revised manuscript, we added Section 2.7, “Statistical metrics”, and provided the equations for all statistical metrics (including dRMSE) used in this study.

#### **Minor Comment #11**

Lines 342-357: The authors should avoid simply listing numerical results that are already visible in Table 2. Also, the text should explicitly guide the reader to the relevant Figures and Tables.

#### **Response #11**

Thank you for pointing out that the original text in this part simply repeated numerical values already shown in Table 2 and did not explicitly guide the reader to the relevant figures and tables. We have rewritten this paragraph to reduce item-by-item repetition of values that are directly visible in the table. Instead, we now emphasize the key contrasts and main conclusions, and we added explicit references to the corresponding table, figure, and subpanels at the appropriate places so that readers can quickly locate the supporting evidence. In addition, we reviewed the entire manuscript and consistently improved the way figures and tables are cited. We aimed to maintain a clear “conclusion–evidence” linkage throughout the results and discussion, thereby improving the overall coherence and readability of the narrative.

#### **Minor Comment #12**

Lines 376-388: This paragraph should be removed or substantially revised. It currently provides little persuasive insight.

### **Response #12**

Thank you for noting this weakness. We have removed this paragraph.

### **Minor Comment #13**

Line 415: (a-h) should be Figure 5 (a-h).

### **Response #13**

Thank you for pointing out this unclear wording. This part has been removed in the revised manuscript. However, during the analysis, we checked all references to subfigures elsewhere to ensure that the subfigure numbering is correct.

### **Minor Comment #14**

Line 443: The instruction to "interpolate to the altitude of the TC center" is conceptually problematic for PWV, as it is a vertically integrated, two-dimensional variable (lat-lon).

### **Response #14**

Thank you for pointing out that the instruction to "interpolate to the altitude of the TC center" is conceptually problematic. We fully agree with your assessment that PWV is a two-dimensional variable that represents a vertically integrated water vapor column and varies with latitude and longitude, so the concept of a "typhoon-center altitude" should not be introduced. In the original description of our method, we also inappropriately implied that PWV at the surrounding grid points should be adjusted to the terrain elevation at the TC center before performing horizontal interpolation. Following your comment, we recognize that this wording, and the corresponding idea, is not appropriate. PWV is a column-integrated variable, and it does not require such a vertical adjustment. Therefore, we have removed the phrase "interpolate to the altitude of the TC center" and the related vertical-adjustment description in the revised manuscript. We have also added the bilinear horizontal interpolation formula and explanation for collocating reanalysis PWV with the observation locations in Section 2.6, to ensure that the method description is clear and rigorous in both physical meaning and implementation.

The above provides our general responses and point-by-point responses to your review comments (both major and minor comments). On behalf of all authors, I would like to sincerely thank you again for the thorough review and constructive suggestions, which helped us identify and correct imprecise parts of the manuscript and improve its clarity and scientific rigor. We have implemented the corresponding revisions throughout the revised manuscript, and we sincerely hope that this revision adequately addresses your concerns. If you feel that any issues remain or have further

suggestions, we would be very happy to revise the manuscript further. Thank you again for your time and careful review.

Sincerely,

Jiaqi Shi

GNSS Research Center, Wuhan University

On behalf of all co-authors

January 2026