Response Letter to Reviewer #1

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 dataset used in this study. In Comment 11, the reviewer suggested that we use typhoon information from the China Meteorological Administration (CMA). In addition, another reviewer raised a similar concern and suggested that typhoon data from either CMA or the International Best Track Archive for Climate Stewardship (IBTrACS) could be used. Following these suggestions, we downloaded and carefully examined the Best Track Data provided by the CMA Tropical Cyclone Data Center under National-level Operational Data Product (https://tcdata.typhoon.org.cn/en/zjljsjj.html). The CMA best track dataset covers our study period (January 2020 to December 2024) and includes most of the typhoon-related information required for this work. Meanwhile, another reviewer noted that using a fixed spatial collocation window may be inappropriate and recommended refining the criterion for determining whether a station is affected by a typhoon by using a dynamic, typhoon-size-dependent standard, such as one based on wind radius. Implementing this approach requires typhoon wind radius that are consistent with the track dataset. To verify the availability of such data, we contacted CMA researchers by email and were informed that the relevant wind-radius information is provided in the TC Size Analysis product under Scientific Research Data Products (https://tcdata.typhoon.org.cn/en/tcsize.html). We further checked both General description of the retrieved Tropical Cyclone Size Dataset (v3.0) and General description of the retrieved Tropical Cyclone Size Dataset (v2.0), and confirmed that these datasets include wind radius information. However, the TC Size Analysis dataset is currently available only up to 2020, and therefore does not cover our full study period (2020–2024). As a result, we are not able to rely on the CMA products to implement the typhoon-size-dependent collocation scheme consistently across the full study period. After carefully considering the reviewers' suggestions, we therefore adopted the IBTrACS dataset to ensure a consistent analysis framework for 2020–2024. Details of the IBTrACS data and our implementation are provided below in our point-by-point response to Comment 11.

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

We substantially revised the results section and added more detailed explanations throughout. In addition, we carefully checked the reference list and in-text citations, and corrected cases where references were inaccurate, inappropriate, or redundant. We further 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 and expanded the analysis 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 wherever the original text was ambiguous or insufficiently justified. Below, we provide our point-by-point responses.

Point-by-point responses:

Comment #1

In line 34, the statement, "The spatio-temporal variation and distribution of PWV does not only influence the vertical humidity structure", is scientifically imprecise. After examination, the provided citations (Kim et al., 2022; Liu et al., 2023) do not appear to support this specific claim. The authors should revise this sentence for scientific accuracy and ensure that the cited literature directly substantiates the point being made.

Response #1 (Line 35–37)

Thank you for pointing out that this statement was not scientifically precise. Our original wording "The spatio-temporal variation and distribution of PWV does not only influence the vertical humidity structure" was indeed inaccurate and overly absolute. We have revised it to: “The spatio-temporal variation and distribution of PWV (also referred to as column water vapor) is related to atmospheric moisture vertical structure, tropical deep convection, and precipitation”. In particular, we replaced “influence” with “is related to” to avoid implying a causal relationship that is not fully supported in this context. We also carefully re-checked the references used to support this statement. As a result, we removed Kim et al. (2022) and Liu et al. (2023), which did not appropriately support the revised claim, and added two more suitable references. Regarding Kim et al. (2022), our previous citation was inappropriate because the paper focuses on precipitable water rather than precipitable water vapor. Regarding Liu et al. (2023), the study primarily examines how the transport height of water vapor affects heavy rainfall associated with tropical cyclones, which is less directly relevant to the statement made here. The reasons for including the two newly added references (and the supporting wording extracted from them) are provided below:

1. Muller et al. (2009): “Rainfall and column-integrated water vapor are closely related in the tropics” in the first sentence of Introduction.
2. Holloway and Neelin (2009): “The first vertical principal component of specific humidity is very highly correlated with column water vapor (CWV) and has a maximum of both total and fractional variance captured in the lower free troposphere (around 800 hPa).” in the second sentence of Abstract. Moreover, the title of this paper is “Moisture Vertical Structure, Column Water Vapor, and Tropical Deep Convection”, and the entire paper discusses the relationship between Moisture Vertical Structure, Column Water Vapor, and Tropical Deep Convection.

Comment #2

In line 35, the citation to "Kim et al., 2022" is ambiguous. Based on the reference list, this should likely be distinguished as "Kim et al., 2022a" or "Kim et al., 2022b". Please verify and correct this instance and all subsequent citations to this literature.

Response #2

Thank you for pointing out the ambiguous citation of Kim et al. (2022). This error occurred because, in the original manuscript, we cited two different 2022 papers whose first authors share the same surname, Kim. One paper is by Kim Seokhyeon et al., “Linking total precipitable water to precipitation extremes globally”. This reference was originally used to support the statement, “The spatio-temporal variation and distribution of PWV does not only influence the vertical humidity structure”. However, based on your Comment #1 regarding the inaccuracy of the supporting literature, and after re-evaluating this part of the discussion, we removed this citation.

The other paper is by Kim Hye-Ji et al., “Comparison of tropical cyclone wind radius estimates between the KMA, RSMC Tokyo, and JTWC”. We cited this study when our initial collocation scheme adopted a fixed spatial window of 300 km. The window size was informed by studies on the wind radius of TC, including Kim Hye-Ji et al. After considering another reviewer’s suggestion to use a dynamic collocation spatial window, we no longer cite this reference. In addition, we reviewed the citation formatting for references published in the same year and with similar author names, and we corrected instances where the citations could be ambiguous.

Comment #3

In line 36, cyclones should be capitalized to give abbreviations: TCs.

Response #3 (Line 38)

Thank you for this comment. We have revised “Tropical cyclones (TCs)” to “Tropical Cyclones (TCs)” by capitalizing the initial letter of “Cyclones.”

Comment #4

In line 37, like the C2 for Wang et al., 2020.

Response #4 (Line 39)

Thank you for pointing out the citation issue with Wang et al. (2020). We have distinguished the two 2020 references as Wang et al. (2020a) and Wang et al. (2020b), corresponding to Wang Linlin et al. (2020) and Wang Shuaimin et al. (2020), respectively. This revision resolves the ambiguity in the in-text citations.

Comment #5

In line 40, the term "translational speed" should be corrected. The standard and more formal term used in the field for the movement of a typhoon is "translation speed".

Response #5 (Line 42)

Thank you for the terminology correction. We have replaced “translational speed” with the more commonly used term in this field, “translation speed”, when referring to TC motion. We also checked the entire manuscript and confirmed that this term appears only in this instance.

Comment #6

In line 40, although the climate trends are observed and modelled, the specific numbers describing the reduction of translation speed and increase of precipitation intensity are not verified in referred literatures. Please check it.

Response #6 (Line 44)

Thank you for this comment. We revisited the references cited in the original manuscript, namely Elsner (2020) and Tran et al. (2022). As you correctly noted, although climate trends have been observed and modeled, the specific quantitative values describing the reduction in tropical cyclone translation speed and the increase in precipitation intensity were not verified by those citations. Indeed, neither of these two papers explicitly provides the specific numbers we reported. We therefore added two additional references to support these quantitative statements. We also removed the citation to Intergovernmental Panel on Climate Change (IPCC) (2022). We initially cited this report to provide the broader context of global warming, but the focus of this sentence is not on global warming itself. In addition, we removed Tran et al. (2022) because its analysis is limited to Southeast Asia and cannot represent global changes in tropical cyclone translation speed and intensity. The three references that support the revised statements are summarized below.

1. Kossin (2018): The title of this paper is "A global slowdown of tropical-cyclone translation speed". Its abstract states that "Here I show that tropical-cyclone translation speed has decreased globally by 10 per cent over the period 1949–2016, which is very likely to have compounded, and possibly dominated, any increases in local rainfall totals that may have occurred as a result of increased tropical-cyclone rain rates", which confirms the 10% global reduction in translation speed over 1949–2016.
2. Elsner (2020): The title of this paper is "Continued increases in the intensity of strong tropical cyclones". The abstract notes that "Oceans have continued to warm since that paper was published, so the intensity of the strongest cyclones should have continued upward as well", indicating a continued increase in the intensity of the strongest cyclones.
3. Knutson et al. (2020): The title of this paper is "Tropical cyclones and climate change assessment". In the Conclusions, the second key point states that "For near-storm TC precipitation rates, there is at least medium-to-high confidence in an increase at the global scale. A representative quantitative estimate for the increase in TC precipitation rates is about 14% for a 2 °C global warming, or close to the rate of tropical water vapor increase expected for atmospheric warming at constant relative humidity". This supports our revised statement that "TC precipitation rates are projected to increase, with a global median increase of ~14% across models".

Comment #7

In line 48, the description of the locations impacted by recent typhoons is geographically inaccurate. The precipitation extremes caused by Typhoon In-fa (2106) and Typhoon Doksuri (2306) located in Henan Province and Beijing-Tianjin-Hebei region correspondingly rather than northern and northeastern China.

Response #7 (Line 50)

Thank you for pointing out the incorrect description of the affected regions for several recent typhoons. We have revised the affected area in the original manuscript from "northern and northeastern China" to "Henan Province and the Beijing–Tianjin–Hebei region", respectively.

Comment #8

In line 70, the statement explaining the sources of differences among reanalysis datasets is incomplete. It correctly identifies "data assimilation strategies" but omits an equally critical factor: the underlying numerical models themselves.

Response #8 (Line 72)

Thank you for pointing out that our explanation of the differences among the reanalysis datasets was incomplete. In addition to "data assimilation strategies," we have added "the underlying numerical models" to provide a more complete explanation.

Comment #9

In line 80, an ambiguous citation format is used again.

Response #9 (Line 82)

Thank you for pointing out again that the citation was ambiguous. We have distinguished the two 2019 references as Zhang et al. (2019a) and Zhang et al. (2019b), corresponding to Weixing Zhang et al. (2019) and Yonglin Zhang et al. (2019), respectively.

Comment #10

In line 103, redundant line.

Response #10

Thank you for pointing out this error. The redundant line has been deleted.

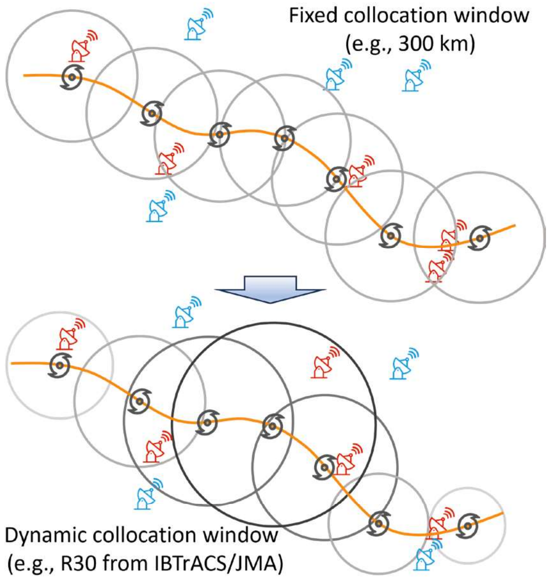
Comment #11

For a scientific study of typhoon track, it is standard practice to use the official "best track" data (e.g., https://tcdata.typhoon.org.cn/zjljsjj.html).

Response #11

Thank you for your comment on the typhoon track data. We provided an overall response to this issue in the General Response. Here we address it in more detail. Following your suggestion, we downloaded and carefully examined the best track dataset provided by the CMA Tropical Cyclone Data Center, which was also recommended by another reviewer. In addition, the second reviewer suggested using a dynamic collocation window related to typhoon wind radius. This makes wind radius information necessary for our revised collocation strategy. However, the CMA best track product does not include wind radius variables required for the typhoon-size-dependent collocation. CMA provides wind radius in a separate TC Size Analysis product, but it is currently available only up to 2020 and therefore does not cover our full study period (2020–2024). Consequently, we were unable to implement a consistent wind- radius -based collocation scheme using CMA products for the full analysis period. Although we would have preferred to use CMA data, we adopted IBTrACS to ensure temporal consistency.

Below we provide further details on the IBTrACS data we used and the implementation of the dynamic collocation. For the western North Pacific, the 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 radius JMA provides two variables, 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 radius of winds of 30 kt or greater. We took the average of TOKYO_R30_LONG and TOKYO_R30_SHORT as the effective influence radius for the typhoon center and used it to collocate the typhoon with GNSS stations, radiosonde stations, and RO profiles. 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. The red stations indicate ground-based stations collocated with the typhoon track, whereas the blue stations indicate ground-based stations not collocated with the typhoon track.



Finally, because we changed the tropical cyclone dataset and re-ran all experiments in this study, some values differ slightly from those reported in the original manuscript but the main results have not changed.

Comment #12

In line 130, another ambiguous citation and repeated literatures in reference list.

Response #12 (Line 133)

Thank you again for your patience and for pointing out that our reference formatting was ambiguous. In the original manuscript, we mistakenly cited Hersbach et al. (2020) twice. We have corrected this error.

Comment #13

In line 131, the description of the ERA5 data assimilation system as simply "four-dimensional variational (4D-Var)" is an oversimplification. For technical accuracy, it should be specified that ERA5 employs a more advanced ensemble 4D-Var system.

Response #13 (Line 134)

Thank you for your suggestion regarding the technical accuracy of the ERA5 data assimilation system. We have revised "four-dimensional variational (4D-Var)" to "an ensemble-based four-dimensional variational (4D-Var)".

Comment #14

In line 137, similar to C12.

Response #14 (Line 140)

Thank you again for pointing out the citation error. We have corrected the duplicate citation.

Comment #15

In line 193, what is the meaning of "a minimum of five standard pressure levels above the surface"?

Response #15 (Line 192–193)

Thank you for pointing out that our description of the radiosonde screening criterion was unclear. Our intention was to ensure that the selected humidity profiles both reach at least 300 hPa and have sufficient vertical sampling for reliable PWV calculation. The original wording was meant to indicate that a profile should contain an adequate number of measurements across the standard pressure levels above the surface (e.g., 1000, 850, 700, 600, 500, 400, and 300 hPa), but we agree that it could be misinterpreted. To avoid ambiguity, we have removed the original phrasing and replaced it with a clearer and reproducible criterion. We now state that "Profiles must reach at least 300 hPa, and only profiles with at least 30 vertical levels are used for PWV calculation". This threshold was chosen empirically and is consistent with common quality-control practices in prior radiosonde-based studies, as it helps retain profiles with sufficiently high vertical resolution while excluding sparsely sampled soundings.

Comment #16

In line 214, The term ρw lacks a definition in the main text.

Response #16 (Line 206)

We have added the definition of ρw in the revised manuscript. Here, ρw denotes the density of liquid water and is set to 1000 $kg\cdot m^{-3}$.

Comment #17

In Figure 2 legend, the notations REA-PWV_C and REA-PWV_I are used in the figure and caption but are not defined. While the text defines GNSS-PWV with subscripts for the CMONOC and IGS networks, the use of the reanalysis is confusing.

Response #17 (Line 286–292)

Thank you for pointing out the confusion caused by the undefined notations in the figure legend and caption. We have added clear definitions of REA-PWV_C and REA-PWV_I in the caption of Figure 3 (Figure 2 in the original manuscript). Here, the subscripts C and I denote the reanalysis PWV at CMONOC and IGS stations, respectively. We use this notation because stations from these two GNSS networks differ in their spatial distribution, and it is therefore necessary to present and discuss the results separately. To further avoid confusion, we have also checked the main text and ensured that these notations are used consistently throughout the manuscript.

Comment #18

In section 3.1.1. Throughout the results section, the discussion of multi-panel figures would be significantly improved by consistently referencing specific subplots (e.g., "Fig. 2a," "Fig. 3d"). Currently, the text makes detailed quantitative statements without navigating the reader to the evidence, forcing them to search. For example, in Section 3.1.1, the descriptions of bias and RMSE should be explicitly linked to panels (b1-b3) and (c1-c3). Furthermore, a specific instance of this lack occurs on line 343, where the statement regarding M-bias for L4 typhoons is made without reference to the supporting figure panel. As a general note on presentation, the subplot labeling scheme itself (e.g., 'a1', 'b1') is unconventional; a standard sequential alphabetic scheme (a, b, c, d) is strongly recommended for clarity and adherence to publication norms.

Response #18

Thank you for this very valuable suggestion. We fully agree that, when discussing multi-panel figures, the text should consistently and explicitly refer to the corresponding subpanels so that readers can quickly locate the evidence and verify the quantitative statements. Following your recommendation, we have made the revisions below:

1. Standardizing subplot labeling. We replaced the previously unconventional subplot labels (e.g., "a1" and "b1") with a more standard sequential alphabetic scheme (a, b, c, d, …). This labeling convention has been applied consistently to all multi-panel figures throughout the manuscript to improve clarity and readability.
2. Adding explicit subplot references in the text. We carefully reviewed and revised the descriptions of figures and tables across the manuscript. For key quantitative results (e.g., bias and RMSE), we now include the corresponding figure and subpanel identifiers at the appropriate places in the text (e.g., Fig. Xa, Fig. Xb). This allows readers to directly navigate to the supporting panels without searching across multi-panel figures, thereby improving the overall reading experience.

Comment #19

In line 377 (major scientific concern). The manuscript's central attribution for JRA-3Q's improved PWV accuracy—the assimilation of tropical cyclone bogus (TCB) data—is physically unsubstantiated. As the authors' own reference (Kosaka et al., 2024) states, the TCB data used in JRA-3Q constrains only dynamical fields (sea level pressure and winds) and contains no humidity information. There is no direct pathway for this data to improve the moisture field. The authors must either provide a rigorous, physically-based hypothesis for how the dynamical constraints indirectly improve PWV and support it with further analysis, or this unsubstantiated claim should be removed.

Response #19

Thank you for raising this critical and insightful scientific concern. We fully agree that our original statement attributing JRA-3Q's improved PWV accuracy directly to the assimilation of tropical cyclone bogus (TCB) data was not physically well supported and could be misleading. Since TCB assimilation primarily constrains dynamical variables, such as sea level pressure and winds, and does not directly provide humidity information, it is not appropriate for us to retain this speculative explanation without further targeted mechanism diagnosis and supporting evidence. In addition, Reviewer #2 also recommended deleting or substantially revising this paragraph. Considering both reviewers' comments, and given that the main objective of this study is a systematic accuracy evaluation of PWV from multiple reanalyses under typhoon conditions rather than an in-depth discussion of assimilation mechanisms, we have removed the related attribution from the revised manuscript and retained only the objective descriptions that are directly supported by our analyses and the necessary background information. We sincerely appreciate your careful review and helpful guidance, which have improved the scientific rigor of the manuscript.

Comment #20

In line 381, The citations to (Liu et al., 2000; Koster et al., 2016) appear in the text, but the full entries are missing from the reference list. Please add the complete reference details for these sources.

Response #20

Thank you for pointing out the missing reference entries. We have checked the citations to Liu et al. (2000) and Koster et al. (2016) in the original manuscript. Because we removed the paragraph containing this discussion in response to the previous major comment (Comment #19), these two studies are no longer cited in the revised manuscript. Accordingly, their full entries are no longer needed in the reference list.

Comment #21

In line 444, what's the meaning of "altitude of the typhoon center"? In my opinion, the typhoon center is located using latitude and longitude, regardless of height.

Response #21

Thank you for pointing out this mistake. We agree that our wording in this part was unclear and could be misleading. By "altitude of the typhoon center," we did not mean that the typhoon center has a physical height. We intended to refer to the terrain elevation at the typhoon center location. 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 typhoon 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 quantity of atmospheric water vapor, and it does not require such an elevation adjustment. Therefore, we have removed the phrase "altitude of the typhoon center" and the related vertical adjustment description in the revised manuscript. We have also added the bilinear interpolation formula for horizontal interpolation in Section 2 and clarified the description of the interpolation procedure accordingly.

The above constitutes our general response and point-by-point responses to your review 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 improved its clarity and scientific rigor. We have implemented these revisions in 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 again. 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

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 30–103)

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 108–120)

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 243–248)

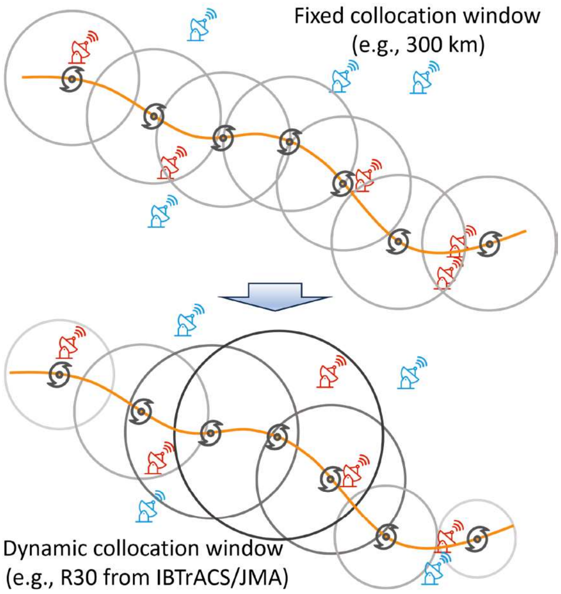
Thank you for your comment on the interpolation method. In the revised manuscript, we have added a clear description in Section 2.6, “Data collocation schemes and PWV horizontal interpolation method”, explaining how REA-PWV is obtained from the reanalysis datasets at GNSS stations, radiosonde stations, 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 232–242)

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 422–434)

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 125–129)

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 159)

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 286–292 & 326–330)

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-PWVc and REA-PWVi should be explained. Is this primarily due to the spatial distribution of GNSS stations from these two networks?

Response #4 (Line 295–297)

Thank you for your concern. Yes, we agree with your view that the differences between REA-PWV_C and REA-PWV_I 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 308–310)

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 314)

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 325)

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 254)

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