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 34–36)

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 37)

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 38)

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 41)

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 43)

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 49)

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 71)

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 81)

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

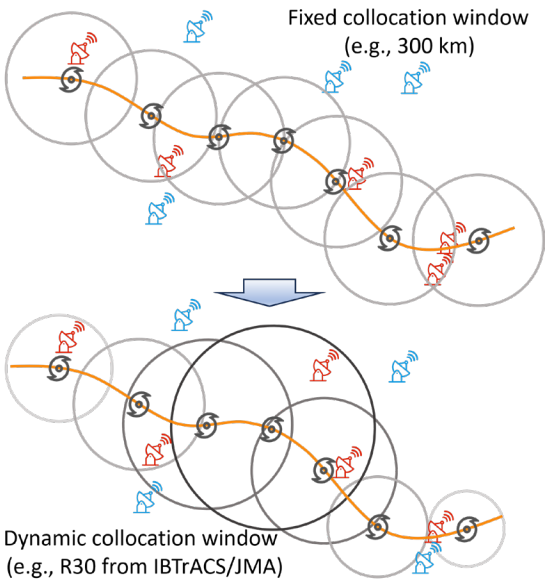
For a scientific study of typhoon track, it is standard practice to use the official "best track" data (e.g., https://tcdata.typhoon.org.cn/zjljsjj.html).

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 132)

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 133)

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 139)

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 191–192)

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 205)

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 285–291)

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