Responses to Referee 2

Summary: The authors thank the referee for their constructive feedback and suggestions. We have made extensive revisions to better contextualize the scientific value and relevance of this work—both in the analyses and in information on the retrievals— with other studies that use RO or PRO data.

A summary of major changes and an overview of how these address the referee's broader concerns can be found below. For brevity, we refer to sections as S (Section 4 is S4), and to lines as "L" (line XXX is LXXX). Each specific comment from the referee has been included in italics and then we specify how it was addressed in detail under "Responses" Unless otherwise noted, the line numbers in this response correspond to those in the revised version with the changes marked.

Responses to referee comments

1. *Line 27: better to have units for k_1 and k_2.*

   Response: k_1 and k_2 are empirically determined proportionality constants that are provided without units in the literature (e.g., [4, 7, 8]), even it makes sense that k_1 and k_2 differ by a ratio of temperature units in K as we now mention in L43-44.

2. *Line 40: 1DVAR (e.g., UCAR/COSMIC) should be briefed here when talking about "wet" profiles, i.e., temperature and humidity.*

   Response: The dataset used in this study relies on JPL retrievals, which apply the direct method [2] rather than the 1D-Var method. However, we also added a reference to the 1D-Var method in L64-68.

3. *Line 53, I think there is another important paper by Padullés et al. (2022) in which they discussed the sensitivity of PRO simulation to frozen hydrometeors based. Could the authors provide any insights into the separation of frozen hydrometeors rather than the two water phases (ice and precipitation) used? Perhaps a few sentences could be added to the discussion or introduction.*

   Additionally, what are the authors' thoughts on how their results can be generalized to high-resolution NWP model states? Specifically, could the authors offer perspectives on the potential application of their methods using NWP model data, rather than the GPM data used in this study?

   Response: The analysis in [5] focused on the RO rays above the freezing temperature level. It remains hard to untangle the structure of water phases along the RO ray, since $\Delta\Phi$ is an integrated value along a ray path that can cross both liquid and ice phases. Doing so is beyond the scope of this paper. It remains an open question how or even if it is possible to differentiate between ice and liquid water precipitation using PRO data alone.

   L89-95 in the introduction discuss how PRO data can be used to distinguish between ice and liquid precipitation, as well as precipitating vs. non-precipitating features, and how some questions are still open.

   The authors do not feel equipped to answer the second question. L69-71 briefly refer to PRO being most valuable where they disagree with NWP models—that is why this study did not use NWP data—but a follow-on effort using NWP instead of GPM and limited to the cases where PRO and NWP refractivities agree, would have increased statistics. In parallel, this combination of NWP and PRO could hold relevance for assimilating PRO data into NWP frameworks [3, 6, 10]. This remains a work in progress, and several preliminary studies on PRO data assimilation in NWP were presented at the 2nd PAZ-Polarimetric Radio Occultations User Workshop and summarized in [9].

4. *Line 60: "Part of the challenge is that a given $\Delta\Phi$ at a specific height may be caused by both ice or precipitation." Any other potential causes? Could the authors provide some discussion about the challenges in representing $\Delta\Phi$ by model states?*

   Response: The wording of "ice or precipitation" has been changed to refer to the separation of ice and liquid water precipitation. To answer the first question, L89-95 have also added some words on the documented influence of anisotropic ice crystals on PRO [5] and the negligible influence of smaller particles like aerosols and non-precipitating cloud droplets on GNSS PRO [3].

5. *Line 66: The k-means cluster analysis is present without introduction. Could the authors provide a brief introduction. What are the major advantages using this analysis? Why do the authors use this method? Any references?*

   Response: L97-101 now have added an introductory paragraph before the former line 66. This paragraph briefly defines cluster analysis and cites several papers, a survey, and a textbook that demonstrate the use of *k*-means clustering in analyzing climate and atmospheric data.

6. *Line 90: It is better to provide a brief introduction about the GPM data. For instance, whether it is gridded data. What are the spatial and temporal resolution? How the matching is being done?*

   Response: L121-124 have added a description of the spatial and temporal resolution of the Level 2 GPM data. A sentence has been added in L136-139, after former lines 87-88, to describe the collocation criteria used for matching:

7. *Line 95 and Fig. 1b: What caused the missing period in Jan/Feb 2019? If the monthly coverage is not equal and the seasonality is not studied in this paper, Fig. 1b does not seem useful and can be removed.*

   Response: We added a footnote to L141 explaining that technical issues with the processing of ROHP-PAZ retrievals from January and February 2019 prevented the creation of the collocated dataset between GPM and ROHP-PAZ for these two months. Although the ROHP-PAZ retrievals from this period have since been corrected and are now available in the latest ROHP-PAZ dataset, the analysis in this study was completed before updated collocations could be made. It does not change the conclusions of our study.

   However, we included Figure 1(b) to provide comprehensive documentation of the dataset, including the seasonal distribution of the coincidences (e.g., a seasonal bias). Since the main text includes a plot of the latitudinal distribution of the dataset (Figure 1(a)), both convey the spatiotemporal distribution of the dataset.

8. *Line 197: Do the authors really mean 300hpa? 300hpa of water vapor pressure is way beyond the quality. How does this the QC threshold of "nonphysically high water vapor pressure values" come from?*

   Response: To the first question, the authors have added in L169-174 a new description of the technique used to identify profiles with unphysically large water vapor pressure profiles in Section 2. It was misleading to emphasize 300 hPa as an a priori threshold in the original text instead of merely stating that 300 hPa, which could have been set to 250 hPa, was an *observed* lower bound for the maximum water vapor pressure in anomalous profiles. This inaccurate wording has been removed.

   As described in the response to comment 2 above, the water vapor pressure in this study is derived from RO refractivity using the direct method [2]. This occasionally leads to unphysical negative values that help identify profiles in which there was an inconsistency between the model and RO observed refractivity. Negative or otherwise unrealistic values serve as a quality control flag. One result of the cluster study was to discover that cluster analysis separated a group associated with unphysical retrievals.

9. *Line 228: "Bretherton et al. (2004) showed a relationship between precipitation and total column water vapor over the tropics." What did this paper say? Could the authors add the main findings of such "relationship?"*

10. *Line 231: Could the authors add a bit more details about the "pickup?" was there any particular pickup values discussed in these papers?*

    Response (to both (9) and (10)): L304-312 have been expanded to summarize the relevant result from [1], an exponentially increasing relationship between precipitation and total column relative humidity over the tropics. [1] does not directly prescribe a "start" to the relationship they identify.

11 *Line 235 and Fig. 3: 1) Besides caption, a brief description should be given for each figure in the content before they are being discussed. 2) if the authors really think they want to talk about Panel d first, they may want to re-arrange the panels so they can discuss by alphabetic orders in the content. 3) As the authors state that "we look for the precipitation pickup pattern." The pattern is not actually being discussed.*

Response:

(a) We have added a brief introduction and description of all panels for Fig. 3 in the main body of the text.

(b) In the revised manuscript, the authors no longer talk about Fig. 3(d) first, but Fig. 3 as a whole.

(c) We have drastically restructured and changed the conclusions of Section 4.1; in the revised manuscript, the precipitation pickup pattern is no longer the focus of Fig. 3, but rather a more general, positive relationship between accumulated $\Delta\Phi$ and total column water vapor. The statistics in the tables 4 show some correlation in the mean values, but the figures are insufficient to convincingly claim that a precipitation pickup pattern is clearly discernible in Fig. 3.

12 *Line 238: "There is also an apparent total column water vapor threshold after which $\Delta\Phi$, the PRO signature of precipitation, starts increasing at a faster rate..." I suggest the authors merge/reorganize this sentence with the previous paragraph where they discussed the relationship in literatures between total column water vapor threshold and $\Delta\Phi$.*

Response: In rewritting Section 4.1, we deleted that sentence (former line 238).

13 *Line 279: Again, this paragraph descripting what Fig. 4 presents should have appeared much earlier.*

Response: We have moved the paragraph describing Fig. 4 from the end of S4.2 to the beginning of S4.2, just before Fig. 4 is first referenced and interpreted.

14 *Line 57: what does "CloudSat" mean particularly here? Any description?*

Response: L83-85 briefly introduce CloudSat.

15 *Line 79: model (3)?*

Response: The phrase "model (3)" within the former line 79 has been rewritten as, "potential refractivity model."

References

[1] C. S. Bretherton, M. E. Peters, and L. E. Back. "Relationships between Water Vapor Path and Precipitation over the Tropical Oceans". In: Journal of Climate 17.7 (Apr. 2004), pp. 1517–1528. DOI: 10.1175/1520-0442(2004)017<1517:rbwvpa>2.0.co;2.

[2] G. Hajj et al. "A technical description of atmospheric sounding by GPS occultation". In: Journal of Atmospheric and Solar-Terrestrial Physics 64.4 (2002), pp. 451–469. ISSN: 1364-6826. DOI: 10.1016/S1364-6826(01)00114-6.

[3] D. Hotta, K. Lonitz, and S. Healy. "Forward operator for polarimetric radio occultation measurements". In: Atmospheric Measurement Techniques 17.3 (2024), pp. 1075–1089. DOI: 10.5194/amt-17-1075-2024.

[4] A. Kliore et al. "Preliminary Results on the Atmospheres of Io and Jupiter from the Pioneer 10 S-Band Occultation Experiment". In: Science 183.4122 (Jan. 1974), pp. 323–324. DOI: 10.1126/science.183.4122.323.

[5] R. Padullés, E. Cardellach, and F. J. Turk. "On the global relationship between polarimetric radio occultation differential phase shift and ice water content". In: Atmospheric Chemistry and Physics 23.3 (2023), pp. 2199–2214. DOI: 10.5194/acp-23-2199-2023.

[6] B. Ruston and S. Healy. "Forecast Impact of FORMOSAT-7/COSMIC-2 GNSS Radio Occultation Measurements". In: Atmospheric Science Letters 22.3 (2021), e1019. DOI: https://doi.org/10.1002/asl.1019.

[7] E. K. Smith and S. Weintraub. "The Constants in the Equation for Atmospheric Refractive Index at Radio Frequencies". In: Proceedings of the IRE 41.8 (1953), pp. 1035–1037. DOI: 10.1109/JRPROC.1953.274297.

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[9] F. J. Turk et al. "Advances in the Use of Global Navigation Satellite System Polarimetric Radio Occultation Measurements for NWP and Weather Applications". In: Bulletin of the American Meteorol 105.6 (2024), E905–E914. DOI: 10.1175/BAMS-D-24-0050.1.

[10] K.-N. Wang et al. "The Effects of Heavy Precipitation on Polarimetric Radio Occultation (PRO) Bending Angle Observations". In: Journal of Atmospheric and Oceanic Technology 39.2 (Feb. 2022), pp. 149–161. DOI: 10.1175/jtech-d-21-0032.1.