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Constraining microphysics assumptions on the modeling of Atmospheric Rivers using GNSS Polarimetric Radio Occultations

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We sincerely thank the reviewer for their constructive and insightful comments, which have helped us improve both the clarity and robustness of our work. In this revised version, we have made several important changes to enhance the physical consistency and transparency of the methodology.

(1) We calrify that there is no direct coupling between ARTS and WRF for the main part of the analysis. Instead, ARTS is used diagnostically to evaluate which paritcle habits are most compatible with the WRF-derived water content and the best x-parameter obtained when comparing with actual observations of differential phase shift.

(2) Second, the optimization process has been refined and is now carried out at two levels: (i) within each microphysics scheme, to obtain the optimal set of x-parameters; and (ii) across all schemes, to identify the combination of microphysics and x-parameters that minimizes the cost function.

(3) In the comparison between the best x-parameter and those derived from ARTS, we now generate two distinct look-up tables that relate Kdp and WC, each corresponding to a different assumed particle size distribution (PSD). This modification ensures a closer alignment between the scattering properties used in ARTS and the microphysical assumptions in WRF.

General

The manuscript is novel and reports on unique way to assess the representation of hydrometeor habits in numerical weather prediction models.
Abstract and introduction. These sections start off with PRO technical details rather than "why" you are doing this work in the first place. Give a reason, some rationale. Suggest something like this:
Improving the prediction of precipitation intensity remains an elusive goal for the operational weather community. In current cloud resolving forecast models, the introduction of hydrometeors and the associated latent heat release are represented with either convective parameterizations or microphysical parameterizations or both, depending on the resolution of the simulation (Hristova Veleva et al 2021, https://doi.org/10.3390/atmos12020154). This study aims to assess the use of PRO in constraining the choice of microphysical assumptions within models, by exploiting the sensitivity of the PRO technique to the model's forecasted water content and selection of hydrometeor shapes.

The why of the analysis is now included both in the abstract and the introduction.

Section 2.3 is confusing as written with the "x-parameter" terminology. It's a strange sort of ad-hoc choice of terminology for a quantity that is fundamental to the material in the remainder of the article. If I understand your discussion, this is the "A" quantity in Eqn (6).

The A parameter in equation (6) is not the same as the x-parameters. Equation (6) accounts for the contribution of the liquid hydrometeor (rain), that is different from the one used for the frozen ones.

The rain contribution does not account for ARTS information. A and B are two constants that are the same for all the Atmospheric River cases.

Specific

Near Line 30. Suggest this wording: GNSS systems such as GPS transmit in a Right Hand Circularly Polarized (RHCP) state. A PRO-capable RO receiver uses dual orthogonal receive polarizations, horizontal (H) and vertical (V), enabling the measurement of differential phase delay ($\Delta\phi$, defined in Section 2.1 below). This differential delay is induced when the transmitted GNSS signals propagate through nonspherical hydrometeors (such as raindrops) in the atmosphere (Cardellach et al., 2015).

The paragraph was rephrased

Line 54. The work by Murphy et al 2019 was the original work that proposed investigating cloud resolving model microphysics. Also your work by Shu-Ya Chen fpt TC cases predates your study https://egusphere.copernicus.org/preprints/2025/egusphere-2024-3708/). Suggest this wording: Murphy et al (2019) proposed using PRO simulations to examine the model sensitivity to assumptions in its microphysical parameterization. However, PRO data were not available at that time. A recent study by Chen et al (2025, in review) compared observed and simulated ROHP data to examine a small number of tropical cyclones (TC) events. Our work focuses on the much larger number of PRO data from ROHP and Spire that cover Atmospheric River (AR) weather events. Together with PRO simulations, our comparisons aid in determining the extent to which variations in microphysical schemes in AR's can be discerned with PRO observations.

The new reference was added at the end of the section.

Line 58: However, the microphysical schemes implemented in mesoscale models such as WRF do not explicitly provide the required scattering characteristics at the GNSS frequencies (near 1.4 GHz) and limb-viewing angles needed to perform the Kdp computation.

The end of the paragraph was rewritten with the suggested wording.

Line 66. You need to cite the Hotta et al (2024) paper here, it was the first paper to simulate and study ROHP data in several AR cases. As they concluded, the AR cases provided the best agreement with ROHP owing to the nature of the weather event (more widespread, hence less variability along the long ray paths, relative to the TC cases with more variability from convective conditions). This is another reason why AR are good choices of weather events to examine.

The article was referenced.

Near Line 95. Most non-RO people will be unfamiliar with the term "ray path". The word is a construct used to refer to the volume of air that is sampled (discretely in time) by the PRO receiver as the RO rises or sets. I think a cross section figure would be helpful to explain what the path length is. You can provide one or refer to several in the Padulles et al papers, or cite the Figure from the summary paper from the "2023 Polarimetric Radio Occultations Workshop" in BAMS. https://doi.org/10.1175/BAMS-D-24-0050.1

In Figure 1 we have added a panel illustrating the occultation rays of that same observation.

Figure 1. The rays are from which PRO data? Perhaps make this for the case shown in Figure 7 so the reader has an idea of which rays sliced through the storm and where.

The figure was changed to one of the cases in figure 7.

Figure 4 caption. Be more specific, i.e., “Longwave IR brightness temperature image from geostationary satellite data”.

The caption was changed.

Line 180. The term “x-parameters” is confusing. So, the x-parameters are the A and B terms in Eqn (6), for each level of the PRO profile, for each specie? (those relate Kdp and WC). Or are you fixing B for each microphysics type and letting only “A” be the “x-parameter”. Explain this better as you refer to “x” a lot from this point on and is a source of confusion.

A and B do not depend on ARTS. They are constants for all the AR cases. The contribution for rain only depends on the WC, i.e. on WRF.

Line 214. Equation 8. One is used to seeing this type of equation for a data assimilation scheme in a forecast model. In that case, one can envision the “initial estimate” (the model background state). What is the background state (Xb) here, and how is it obtained? While y and H(x) are easy to envision- H(x) is your simulated $\Delta\Phi$ and y is the observed $\Delta\Phi$.

We thank the reviewer for this observation. In our case, we do not employ an initial estimate or “background state” as in classical data assimilation schemes. Instead, we only use the second term of Equation (8), since our approach does not rely on a first guess but rather performs a scan of the entire physically reasonable range of x-parameter values. To avoid confusion, we have removed the first term from the equation in the revised manuscript.

Line 225. From my understanding, you are assuming a diagonal background covariance matrix with all off-diagonal terms set to zero. Is correct, and if so, is this realistic? No need to change, just explain and justify.

No, we are not assuming a background covariance matrix in this analysis, as it is said in the manuscript. But we do consider a diagonal observation covariance matrix. While this is a simplification, it allows the optimization to be tractable and avoids introducing unverified cross-covariances. Since the focus of this article is to use PRO as a diagnostic tool, rather than a full assimilation system, we have considered this assumptions reasonable.

Figure 6. It’s a stretch to see much of a relation here. Why not plot the integrated $\Delta\Phi$ above, say, the -10 C level (where the bulk of the ice is) instead? If there is a relation, it may be better revealed.

The aim of showing this kind of figure is that no general relation is observed between differential phase shift and water content. Instead, the relation will depend on the parameterizations and assumptions made for the different hydrometeors, and the specific atmospheric conditions of each case.

Line 282. You say, “the minimization of J is performed jointly for both the microphysics scheme and x-parameter”. Should not this have been mentioned back when you introduced Eqn (8)?

Around Eq. 8 we have rephrased the paragraph explaining the optimization, to clarify this.

Line 292. You say, “Even slight variations in the snow’s position can lead to differences in the results. However, the large-scale characteristics of atmospheric rivers tend to mitigate these effects.”

For the first point, do you think that this is important, even for AR events? Cite the Hotta et al. 2024 manuscript for this last point.

We have clarified that, while the displacement of snow layers can indeed influence the results, the features of Atmopsheric Rivers structures mitigates the overall impact of such shifts in the optimizations. We now cite Hotta el al. (2024) to support this statement and highlight the robustness of AR large-scale characteristics against small positional errors.